THE AMERICAN
JOURNAL OF PSYCHOLOGY
Founded by G. STANLEY HALL in 1887

VOL. XX JULY, 1909 No. 3

SEXUAL EDUCATION AND NAKEDNESS[1]

By HAVELOCK ELLIS

What is the psychological influence of familiarity with nakedness? How far should children be made familiar with the naked body? This is a question in regard to which different opinions have been held in different ages, and during recent years a remarkable change has begun to come over the minds of practical educationalists in regard to it.

In Sparta, in Chios, and elsewhere in Greece, women at one time practised gymnastic feats and dances in nakedness, together with the men, or in their presence. Plato in his *Republic* approved of such customs and said that the ridicule of those who laughed at them was but "unripe fruit plucked from the tree of knowledge." On many questions Plato's opinions changed, but not on this. In the *Laws*, which are the last outcome of his philosophic reflection in old age, he still advocates the co-education of the sexes, and their co-operation in all the works of life, in part with a view to blunt the over-keen edge of sexual appetite; with the same object he advocated the association together of youths and girls without constraint in costumes which offered no concealment to the form.

It is noteworthy that the Romans, a coarser-grained people than the Greeks, and in our narrow modern sense more "moral," showed no perception of the moralizing and refining influence of nakedness. Nudity to them was merely a licentious indulgence, to be treated with contempt even when it was enjoyed. It was confined to the stage, and clamored for by the popu-

[1] The present article forms a chapter in the sixth and final volume of the author's *Studies in the Psychology of Sex*, "Sex in Relation to Society." (F. A. Davis & Co., Phila., Publishers.)

lace. In the Floralia, especially, the crowd seem to have claimed it as their right that the actors should play naked, probably, it has been thought, as a survival of a folk-ritual. But the Romans, though they were eager to run to the theatre, felt nothing but disdain for the performers. "Flagitii principium est, nudare inter cives corpora." So thought old Ennius, as reported by Cicero, and that remained the genuine Roman feeling to the last. "Quanta perversitas!" as Tertullian exclaimed, "Artem magnificant, artificem notant." In this matter the Romans, although they aroused the horror of the Christians, were yet in reality laying the foundation of Christian morality.

Christianity, which found so many of Plato's opinions congenial, would have nothing to do with his view of nakedness and failed to recognize its psychological correctness. The reason was simple, and indeed simple-minded. The Church was passionately eager to fight against what it called "the flesh," and thus fell into the error of confusing the subjective question of sexual desire with the objective spectacle of the naked form. "The flesh" is evil, therefore "the flesh" must be hidden. And they hid it without understanding that in so doing they had not suppressed the craving for the human form, but on the contrary had heightened it by imparting to it the additional fascination of a forbidden mystery.

Burton, in his *Anatomy of Melancholy* (Part III, Sect. II, Mem. II, Subs. IV), referring to the recommendation of Plato, adds: "But *Eusebius* and *Theodoret* worthily lash him for it; and well they might: for as one saith, the very sight of naked parts *causeth enormous, exceeding concupiscences, and stirs up both men and women to burning lust.*" Yet, as Burton himself adds further on in the same section of his work (Mem. v. Subs. iii), without protest, "Some are of opinion, that to see a woman naked is able of itself to alter his affection; and it is worthy of consideration, saith *Montaigne* the Frenchman in his Essays, that the skillfullest masters of amorous dalliance appoint for a remedy of venereous passions, a full survey of the body."

There ought to be no question regarding the fact that it is the adorned, the partially concealed body, and not the absolutely naked body which acts as a sexual excitant. I have brought together some evidence on this point in the study of "The Evolution of Modesty." It is now generally held that one of the chief primary causes of ornament and clothing was the desire to arouse sexual desire, and artists' models are well aware that when they are completely unclothed they are most safe from undesired masculine advances. "A favorite model of mine told me," remarks Dr. Shufeldt (*Medical Brief*, Oct.,

1904), the distinguished author of *Studies of the Human Form*, "that it was her practice to disrobe as soon after entering the artist's studio as possible, for, as men are not always responsible for their emotions, she felt that she was far less likely to arouse or excite them when entirely nude than when only semi-draped." This fact is, indeed, quite familiar to artists' models. If the conquest of sexual desire were the first and last consideration of life it would be more reasonable to prohibit clothing than to prohibit nakedness.

When Christianity absorbed the whole of the European world this strict avoidance of even the sight of "the flesh," although nominally accepted by all as the desirable ideal, could only be carried out, thoroughly and completely, in the cloister. In the practice of the world outside, although the original Christian ideals remained influential, various pagan and primitive traditions in favor of nakedness still persisted and were to some extent allowed to manifest themselves, alike in ordinary custom and on special occasions.

How widespread is the occasional or habitual practice of nakedness in the world generally, and how entirely concordant it is with even a most sensitive modesty has been set forth in "The Evolution of Modesty," in Vol. I of these *Studies*.

Even during the Christian era the impulse to adopt nudity, often with the feeling that it was an especially sacred practice, has persisted. The Adamites of the second century, who read and prayed naked, and celebrated the sacrament naked, according to the statement quoted by St. Augustine, seem to have caused little scandal so long as they only practised nudity in their sacred ceremonies. The French Picards at a much later date insisted on public nakedness believing that God had sent their leader into the world as a new Adam to re-establish the law of nature; they were persecuted and were finally exterminated by the Hussites.

In daily life, however, a considerable degree of nakedness was tolerated during mediæval times. This was notably so in the public baths frequented by men and women together. Thus Alwin Schultz remarks (in his *Höfischs Leben zur Zeit der Minnesänger*), that the women of the aristocratic classes, though not the men, were often naked in these baths except for a hat and a necklace.

It is sometimes stated that in the mediæval religious plays Adam and Eve were absolutely naked. Chambers doubts this and thinks they wore flesh-colored tights, or were as in a later play of this kind "apparelled in white leather". (E. K. Chambers, *The Mediæval Stage*, vol. I, p. 5). It may be so, but the public exposure even of the sexual organs was permitted, and that in aristocratic houses, for John of Salisbury

(in a passage quoted by Buckle, *Commonplace Book*, 541) protests against this custom.

The women of the feminist sixteenth century in France, as R. de Maulde la Claviere remarks (*Revue de l'Art*, January, 1898), had no scruple in recompensing their adorers by admitting them to their toilette or even their bath. Late in the century they became still less prudish, and many well-known ladies allowed themselves to be painted naked down to the waist, as we see in the portrait of Gabrielle d'Estrèes au Bain, at Chantilly. Many of these pictures, however, are certainly not real portraits.

Even in the middle of the seventeenth century in England nakedness was not prohibited in public for Pepys tells us that on the 29th July, 1667 a Quaker came into Westminster Hall crying "Repent! Repent!" being in a state of nakedness except that he was "very civilly tied about the privities to avoid scandal"

In a Chapter "De la Nudité" and in the appendices of his book *De l'Amour* (vol. I, p. 221) Senancour gives instances of the occasional practice of nudity in Europe and adds some interesting remarks of his own.

The movement of revolt against nakedness never became completely victorious until the nineteenth century. That century represented the triumph of all the forces that banned public nakedness everywhere and altogether. If, as Puda insists, nakedness is aristocratic and the slavery of clothes a plebeian characteristic imposed on the lower classes by an upper class who reserved to themselves the privilege of physical culture, we may perhaps connect this with the outburst of democratic plebeianism which, as Neitzsche pointed out, reached its climax in the nineteenth century. It is in any case certainly interesting to observe that by this time the movement had entirely changed its character. It had become general, but at the same time its foundation had been undermined. It had largely lost its religious and moral character and instead was regarded as a matter of convention. The nineteenth century men who encountered the spectacle of white limbs flashing in the sunlight no longer felt like the mediæval ascetic that he was risking the salvation of his immortal soul or even courting the depravation of his morals; he merely felt that it was "indecent" or, in extreme cases, "disgusting". That is to say he regarded the matter as simply a question of conventional etiquette, at the worst, of taste, of æsthetics. In thus bringing down his repugnance to nakedness to so low a plane he had indeed rendered it generally acceptable, but at the same time he had deprived it of high sanction. His profound horror of nakedness was out of relation to the frivolous grounds on which he based it.

We must not, however, underrate the tenacity with which this horror of nakedness was held. Nothing illustrates more vividly the deeply ingrained hatred which the nineteenth century felt of nakedness than the ferocity—there is no other word for it—with which Christian missionaries to savages all over the world, even in the tropics, insisted on their converts adopting the conventional clothing of Northern Europe. Travellers' narratives abound in references to the emphasis placed by missionaries on this change of custom, which was both injurious to the health of the people and degrading to their dignity. It is sufficient to quote one authoritative witness, Lord Stanmore, former Governor of Fiji, who read a long paper to the Anglican Missionary Conference in 1894 on the subject of "Undue Introduction of Western Ways." "In the centre of the village," he remarked in quoting a typical case, "is the church, a wooden barn-like building. If the day be Sunday we shall find the native minister arrayed in a greenish-black swallow-tail coat, a neckcloth, once white, and a pair of spectacles, which he probably does not need, preaching to a congregation the male portion of which is dressed in much the same manner as himself, while the women are dizened out in old battered hats or bonnets, and shapeless gowns like bathing dresses, or it may be in crinolines of an early type. Chiefs of influence and women of high birth, who in their native dress would look, and do look, the ladies and gentlemen they are, are by their Sunday finery given the appearance of attendants upon Jack-in-the-green. If a visit be paid to the houses of the town, after the morning's work of the people is over, the family will be found sitting on chairs, listless and uncomfortable, in a room full of litter. In the houses of the superior native clergy, there will be a yet greater aping of the manners of the West. There will be chairs covered with hideous antimacassars, tasteless round worsted-work mats for absent flower jars, and a lot of ugly, cheap, and vulgar china chimney ornaments, which, there being no fireplace and consequently no chimney piece, are set out in order on a rickety deal table. The whole life of these village folk is one piece of unreal acting. They are continually asking themselves whether they are incurring any of the penalties entailed by infraction of the long table of prohibitions, and whether they are living up to the foreign garments they wear. Their faces have for the most part an expression of sullen discontent, they move about silently and joylessly, rebels in heart to the restrictive code on them, but which they fear to cast off, partly from a vague apprehension of possible secular results, and partly because they suppose they will cease to be good Christians if they do so. They have good ground for their dissatisfaction. At the

time when I visited the villages I have specially in my eye, it was punishable by fine and imprisonment to wear native clothing, punishable by fine and imprisonment to wear long hair or a garland of flowers; punishable by fine and imprisonment to wrestle or to play at ball; punishable by fine and imprisonment to build a native-fashioned house; punishable not to wear shirt and trousers, and in certain localities coat and shoes also; and, in addition to laws enforcing a strictly puritanical observation of the Sabbath, it was punishable by fine and imprisonment to bathe on Sundays. In some other places bathing on Sunday was punishable by flogging; and to my knowledge women have been flogged for no other offence. Men in such circumstances are ripe for revolt, and sometimes the revolt comes."

An obvious result of reducing the feeling about nakedness to an unreasoning but imperative convention is the tendency to prudishness. This, as we know, is a form of pseudo-modesty which, being a convention, and not a natural feeling, is capable of unlimited extension. It is by no means confined to modern times or to Christian Europe. The Turks are capable of prudishness. So indeed were even the ancient Greeks. "Dion the philosopher tells us," remarks Clement of Alexandria (*Stromates*' Bk. IV, ch. 19), "that a certain woman Lysidica, through excess of modesty, bathed in her clothes, and that Philotera, when she was to enter the bath, gradually drew back her tunic as the water covered her naked parts; and then rising by degrees, put it on." Mincing prudes were found among the early Christians and their ways are graphically described by St. Jeremy in one of his Letters. Whenever a new and artificial "modesty" is imposed upon savages prudery tends to arise. Haddon describes this among the natives of Torres Straits, where even the children now suffer from exaggerated prudishness, though formerly absolutely naked and unashamed. (*Cambridge Anthropological Expedition to Torres Straits*, vol. V, p. 271.)

The nineteenth century, which witnessed the triumph of timidity and prudery in this matter, also produced the first fruitful germ of new conceptions of nakedness. To some extent these were embodied in the great romantic movement. Rousseau indeed had placed no special insistence on nakedness as an element of the return to Nature which he preached so influentially. A new feeling in this matter emerged, however, with characteristic extravagance, in some of the episodes of the Revolution, while in Germany in the pioneering *Lucinde* of Friedrich Schlegel, a characteristic figure in the Romantic movement, a still unfamiliar conception of nakedness was set forth in a serious and earnest spirit.

In England, Blake with his strange and flaming genius, proclaimed a mystical gospel which involved the spiritual glorification of the body and contempt for the civilized worship of clothes ("As to a modern man," he wrote, "stripped from his load of clothing he is like a dead corpse"); while later in America Thoreau and Whitman and Burroughs asserted, still more definitely, a not dissimilar message concerning the need of returning to Nature.

We find the importance of the sight of the body—though very narrowly, for the avoidance of fraud, in the preliminaries of marriage—set forth as early as the sixteenth century, by Sir Thomas More in his *Utopia*, which is so rich in new and fruitful ideas. In Utopia, according to Sir Thomas More, before marriage, a staid and honest matron "showeth the woman, be she maid or widow, naked to the wooer. And likewise a sage and discreet man exhibiteth the wooer naked to the woman. At this custom we laughed and disallowed it as foolish. But they on their part do greatly wonder at the folly of all other nations which in buying a colt, where a little money is in hazard, be so chary and circumspect that though he be almost all bare, yet they will not buy him unless the saddle and all the harness be taken off, lest under these coverings be hid some gall or sore. And yet in choosing a wife, which shall be either pleasure or displeasure to them all their life after, they be so reckless that all the residue of the woman's body being covered with clothes, they estimate her scarcely by one handsbreadth (for they can see no more but her face) and so join her to them not without great jeopardy of evil agreeing together, if anything in her body afterward should chance to offend or mislike them. Verily so foul deformity may be hid under these coverings that it may quite alienate and take away the man's mind from his wife, when it shall not be lawful for their bodies to be separate again. If such deformity happen by any chance after their marriage is consummate and finished, well, there is no remedy but patience. But it were well done that a law were made whereby all such deceits were eschewed and avoided beforehand."

The clear conception of what may be called the spiritual value of nakedness—by no means from More's point of view, but as a part of natural hygiene in the widest sense, and as a high and special aspect of the purifying and ennobling function of beauty—is of much later date. It is not clearly expressed until the time of the Romantic Movement at the beginning of the nineteenth century. We have it admirably set forth in Senancour's *De l'Amour* (first edition 1806; fourth and enlarged edition 1834), which still remains one of the best books on the morality of love. After remarking that nakedness by

no means abolishes modesty, he proceeds to advocate occasional partial or complete nudity. "Let us suppose," he remarks, somewhat in the spirit of Plato, "a country in which at certain general festivals the women should be absolutely free to be nearly or even quite naked. Swimming, waltzing, walking, those who thought good to do so might remain unclothed in the presence of men. No doubt the illusions of love would be little known, and passion would see a diminution of its transports. But is it passion that in general ennobles human affairs? We need honest attachments and delicate delights, and all these we may obtain while still preserving our common sense. . . . Such nakedness would demand corresponding institutions, strong and simple, and a great respect for those conventions which belong to all times." (Senancour, *De l' Amour*, vol. I, p. 314.)

From that time onwards references to the value and desirability of nakedness become more and more frequent in all civilized countries, sometimes mingled with sarcastic allusions to the false conventions we have inherited in this matter. Thus Thoreau writes in his Journal on 12th June, 1852, as he looks at boys bathing in the river: "The color of their bodies in the sun at a distance is pleasing. I hear the sound of their sport borne over the water. As yet we have not man in Nature. What a singular fact for an angel visitant to this earth to carry back in his note-book, that men were forbidden to expose their bodies under the severest penalties!" Yevan Bloch, in Chapter VII of his *Sexual Life of our Times* discusses this question of nakedness from the modern point of view, and concludes: "A natural conception of nakedness: that is the watchword of the future. All the hygienic, æsthetic and moral efforts of our time are pointing in that direction."

Stratz, as befits one who has worked so strenuously in the cause of human health and beauty, admirably sets forth the stage which we have now attained in this matter. After pointing out (*Die Frauenkleidung*, 3rd ed. 1904, p. 30) that, in opposition to the pagan world which worshipped naked gods, Christianity developed the idea that nakedness was merely sexual and therefore immoral, he proceeds: "But over all glimmered on the heavenly heights of the cross, the naked body of the Saviour. Under that protection there has gradually disengaged itself from the confusion of ideas a new transfigured form of nakedness made free after long struggle. I would call this *artistic nakedness*, for as it was immortalized by the old Greeks through art, so also among us it has been awakened to new life by art. Artistic nakedness is in its nature much higher than either the natural or the sensual conception of nakedness. The simple child of nature sees in nakedness

nothing at all; the clothed man sees in the uncovered body only a sensual irritation. But at the highest standpoint man consciously returns to nature, and recognizes that under the manifold coverings of human fabrication there is hidden the most splendid creature that God has created. One may stand in silent worshipping wonder before the sight; another may be impelled to imitate and show to his fellowman what in that holy moment he has seen. But both enjoy the spectacle of human beauty with full consciousness and enlightened purity of thought."

It was not, however, so much on these more spiritual sides, but on the side of hygiene, that the nineteenth century furnished its chief practical contribution to the new attitude towards nakedness.

Lord Monboddo, the Scotch judge, who was a pioneer in regard to many modern ideas, had already in the eighteenth century realized the hygienic value of "air baths" and he invented that now familiar name. "Lord Monboddo," says Boswell in 1777 (*Life of Johnson*, edited by Hill, vol. III, p. 168) "told me that he awaked every morning at four, and then for his health got up and walked in his room naked, with the window open, which he called taking *an air bath*." It is said also, I know not on what authority, that he made his beautiful daughters take an air-bath naked on the terrace every morning. Another distinguished man of the same century, Benjamin Franklin, used sometimes to work naked in his study on hygienic grounds, and, it is recorded, once affrighted a servant girl by opening the door, in an absent-minded moment, thus unattired.

Rikli seems to have been the apostle of air baths and sun baths regarded as a systematic method. He established light and air-baths, over half a century ago, at Trieste and elsewhere in Austria. His motto was: "Light, Truth and Freedom are the motive forces towards the highest development of physical and moral health." Man is not a fish, he declared, light and air are the first conditions of a highly organized life. Solaria for the treatment of a number of different disordered conditions are now commonly established and most systems of natural therapeutics attach prime importance to light and air, while in medicine generally it is beginning to be recognized that such influences can by no means be neglected. Dr. Fernand Sandoz, in his *Introduction à la Thérapeutique Naturiste par les agents Physiques et Diététiques* (1907) sets forth such methods comprehensively. In Germany sun-baths have become widely common; thus Lenkei (in a paper summarized in British Medical Journal, 31. Oct. 1908) prescribes them with much benefit in tuberculosis, rheumatic conditions, obesity, anæ-

mia, neurasthenia, etc. He considers that their peculiar value lies in the action of light. Professor J. N. Hyde, of Chicago, even believes "Light-Hunger in the Production of Psoriasis," *British Medical Journal*, 6. Oct. 1906) that psoriasis is caused by deficiency of sunlight and is best cured by the application of light. This belief, which has not, however, been generally accepted in its unqualified form, he ingeniously supports by the fact that psoriasis tends to appear on the most exposed parts of the body, which may be held to naturally receive and require the maximum of light, and by the absence of the disease in hot countries and among negroes.

The hygienic value of nakedness is indicated by the robust health of the savage throughout the world who go naked. The vigor of the Irish, also, has been connected with the fact that (as Fynes Moryson's Itinerary shows) both sexes, even among persons of high social class, were accustomed to go naked except for a mantle, especially in more remote parts of the country, until the seventeenth century. Wherever primitive races abandon nakedness for clothing, at once the tendency to disease, mortality, and degeneracy notably increases, though it must be remembered that the use of clothing is commonly accompanied by the introduction of other bad habits. "Nakedness is the only condition universal among vigorous and healthy savages; at every other point perhaps they differ", remarks Frederick Boyle in a paper ("Savages and Clothes," *Monthly Review*, Sept., 1905) in which he brings together much evidence concerning the hygienic advantages of the natural human state in which man is "all face."

It is in Germany that a return towards nakedness has been most ably and thoroughly advocated, notably by Dr. H. Pudor in his *Nackt Cultur*, and by R. Ungewitter in *Die Nacktheit* (first published in 1905), a book which has had a very large circulation in many editions. These writers enthusiastically advocate nakedness not only on hygienic but on moral and artistic grounds. Pudor insists more especially that "nakedness, both in gymnastics and in sport, is a method of cure and a method of regeneration;" he advocates co-education in this culture of nakedness. Although he makes large claims for nakedness—believing that all the nations which have disregarded these claims have speedily become decadent—Pudor is less hopeful than Ungewitter of any speedy victory over the prejudices opposed to the culture of nakedness. He considers that the immediate task is education and that a practical commencement may best be made with the foot which is specially in need of hygiene and exercise; a large part of the first volume of his book is devoted to the foot.

As the matter is to-day viewed by those educationalists who

are equally alive to sanitary and sexual considerations, the claims of nakedness, so far as concerns the young, are regarded as part alike of physical and moral hygiene. The free contact of the naked body with air and water and light makes for the health of the body; familiarity with the sight of the body abolishes petty pruriencies, trains the sense of beauty and makes for the health of the soul. This double aspect of the matter has undoubtedly weighed greatly with those teachers who now approve of customs which a few years ago would have been hastily dismissed as "indecent." There is still a wide difference of opinion as to the limits to which the practice of nakedness may be carried, and also as to the age when it should begin to be restricted. The fact that the adult generation of to-day grew up under the influence of the old horror of nakedness is an inevitable check on any revolutionary changes in these matters.

Maria Lischnewska, one of the ablest advocates of the methodical enlightenment of children in matters of sex (*op. cit.*), clearly realizes that a sane attitude towards the body lies at the root of a sound education for life. She finds that the chief objection encountered in such education as applied in the higher classes of schools is "the horror of the civilized man at his own body." She shows that there can be no doubt that those who are engaged in the difficult task of working towards the abolition of that superstitious horror have taken up a moral task of the first importance.

Walter Gerhard, in a thoughtful and sensible paper on the educational question ("Ein Kapitel zur Erziehungsfrage, *Geschlecht und Gesellschaft*, Vol. I, Heft. II), points out that it is the adult who needs education in this matter—as in so many other matters of sexual enlightenment—considerably more than the child. Parents educate their children from the earliest years in prudery and vainly flatter themselves that they have thereby promoted their modesty and morality. He records his own early life in a tropical land and accustomed to nakedness from the first. "It was not till I came to Germany when nearly twenty, that I learnt that the human body is indecent, and that it must not be shown because that would 'arouse bad impulses.' It was not till the human body was entirely withdrawn from my sight and after I was constantly told that there was something improper behind clothes, that I was able to understand this. . . . Until then I had not known that a naked body, by the mere fact of being naked, could arouse erotic feelings. I had known erotic feelings, but they had not arisen from the sight of the naked body, but gradually blossomed from the union of our souls." And he draws the final moral: that, if only for the sake of our children, we must learn to educate ourselves.

Forel (*Die Sexuelle Frage*, p. 140), speaking in entirely the same sense as Gerhard, remarks that prudery may be either caused or cured in children. It may be caused by undue anxiety in covering their bodies and hiding from them the bodies of others. It may be cured by making them realize that there is nothing in the body that is unnatural and that we need be ashamed of, and by encouraging bathing of the sexes in common. He points out (p. 512) the advantages of allowing children to be acquainted with the adult forms which they will themselves some day assume, and condemns the conduct of those foolish persons who assume that children already possess the adult's erotic feelings about the body. That is so far from being the case that children are frequently unable to distinguish the sex of other children apart from their clothes.

At the Mannheim Congress of the German Society for Combating Venereal Diseases specially devoted to sexual hygiene the speakers constantly referred to the necessity of promoting familiarity with the naked body. Thus Eulenburg and Julian Marcus (*Sexualpedagogik*, p. 264), emphasized the importance of air-baths not only for the sake of the physical health of the young but in the interest of rational sexual training. Höller, a teacher, speaking at the same congress (*op. cit.* p. 85), after insisting on familiarity with the nude in art and literature, and protesting against the bowdlerising of poems for the young, continues: "By bathing-drawers ordinances no soul was ever yet saved from moral ruin. One who has learnt to enjoy peacefully the naked in art is only stirred by the naked in nature as by a work of art." Enderlin, another teacher, speaking in the same sense (p. 58), points out that nakedness cannot act sexually or immorally on the child since the sexual impulse has not yet become pronounced, and the earlier he is introduced to the naked in nature and in art, as a matter of course, the less likely are the sexual feelings to be developed precociously. The child thus, indeed, becomes immune to impure influences, so that, later, when representations of the nude are brought before him for the object of provoking his wantonness they are powerless to injure him. It is important, Enderlin adds, for familiarity with the nude in art to be learnt at school; for most of us, as Siebert remarks, have to learn purity through art.

Nakedness in bathing, remarks Bëlsche in his *Libeslehen in der Natur* (vol. III, pp. 139 *et seq.*), we already in some measure possess; we need it in physical exercises, at first for the sexes separately; then, when we have grown accustomed to the idea, occasionally for both sexes together. We need to acquire the capacity to see the bodies of individuals of the other sex with such self-control and such natural instinct that they become

non-erotic to us and can be gazed at without erotic feeling. Art, he says, shows that this is possible in civilization. Science, he adds, comes to the aid of the same view.

Ungewitter (*Die Nachtheit*, p. 57) also advocates boys and girls engaging in play and gymnastics together, entirely naked in air-baths. "In this way", he believes, "the gymnasium would become a school of morality, in which young growing things would be able to retain their purity as long as possible through becoming naturally accustomed to each other. At the same time their bodies would be hardened and devoloped, and the perception of beautiful and natural forms awakened." To those who have any "moral" doubts on the matter he mentions the custom in remote country districts of boys and girls bathing together quite naked and without any sexual consciousness. Rudolf Sommer, similarly, in an excellent article entitled Mädchenerziehung oder Menschenbildung?" (*Gesehlecht und Gesellschaft*, Bd. I, Heft 3) advises that children should be made accustomed to each other's nakedness from an early age in the family life of the house or the garden, in games and especially in bathing: he remarks that parents having children of only one sex should cultivate for their children's sake intimate relations with a family having children of like age of the opposite sex, so that they may grow up together.

It is scarcely necessary to add that the cultivation of nakedness must always be conciliated with respect for the natural instincts of modesty. If the practice of nakedness led the young to experience a diminished reverence for their own or others' personalities the advantages of it would be too dearly bought. This is in part a matter of wholesome instinct, in part of wise training. We now know that the absence of clothes has little relation with the absence of modesty, such relation as there is being of the inverse order, for the savage races which go naked are usually more modest than those which wear clothes. The saying quoted by Herodotus in the early Greek world that "A woman takes off her modesty with her shift" was a favorite text of the Christain Fathers. But Plutarch, who was also a moralist, had already protested against it at the close of the Greek world: "By no means," he declared; "she who is modest clothes herself with modesty when she lays aside her tunic." "A woman may be naked," as Mrs. Bishop, the traveller, remarked to Dr. Baelz in Japan, "and yet behave like a lady."

The question is complicated among ourselves because established traditions of rigid concealment have fostered a pruriency which is an offensive insult to naked modesty. In many lands the women who are accustomed to be almost or quite naked in the presence of their own people cover themselves as

soon as they become conscious of the lustful inquisitive eyes of Europeans. Stratz refers to the prevalence of this impulse of offended modesty in Japan, and mentions that he himself failed to arouse it because he was a physician, and moreover. had long lived in another land (Java) where also the custom of nakedness prevails.[1] So long as this unnatural prurience exists a free unqualified nakedness is rendered difficult or impossible.

Modesty, is not, however, the only natural impulse which has to be considered in relation to the custom of nakedness. It seems probable that in cultivating the practice of nakedness we are not merely carrying out a moral and hygienic prescription but allowing legitimate scope to an instinct which at some periods of life, especially in adolescence, is spontaneous and natural, even, it may be, wholesomely based in the traditions of the race in sexual selection. Our rigid conventions make it impossible for us to discuss the laws of nature in this matter by stifling them at the outset. It may well be that there is a rhythmic harmony and concordance between impulses of modesty and impulses of nakedness, though we have done our best to disguise the natural law by our stupid and perverse by-laws.

Stanley Hall, who emphasizes the importance of nakedness, remarks that at puberty we have much reason to assume that in a state of nature there is a certain instinctive pride and ostentation that accompanies the new local development, and quotes the observation of Dr. Seerley that the impulse to conceal the sexual organs is especially marked in young men who are under-developed but not evident to those who are developed beyond the average. Stanley Hall (*Adolescence*, Vol. II, p. 97) also refers to the frequency with which not only "virtuous young men, but even women, rather glory in occasions when they can display the beauty of their forms without reserve, not only to themselves and to loved ones, but even to others with proper pretexts."

Many have doubtless noted this tendency, especially in women, and chiefly in those who are conscious of beautiful physical development. Madame Celine Renooz believes that the tendency corresponds to a really deep-rooted instinct in women little or not at all manifested in men who have consequently sought to impose artificially on women their own masculine conceptions of modesty. "In the actual life of the young girl to-day there is a moment when by a secret atavism she feels the pride of her sex, the intuition of her moral superiority and cannot understand why she must hide its cause. At this mo-

[1] C. H. Stratz: *Die Körperformen en Kunst und Leben der Japoner*, 2nd ed. ch. III; *id. Frauenkleidung*, *3rd* ed., pp. 22, 30.

ment, wavering between the laws of nature and social conventions, she scarcely knows if nakedness should or should not affright her. A sort of confused atavistic memory recalls to her a period before clothing was known, and reveals to her as a paradisiacal ideal the customs of that human epoch." (Celine Renooz, *Psychologie Comparée de l'Homme et de la Femme*, pp. 85-87.)

From the point of view with which we are here essentially concerned there are three ways in which the cultivation of nakedness—so far as it is permitted by the slow education of public opinion—tends to exert an influence. (1) It is an important element in the sexual hygiene of the young, introducing a wholesome knowledge and incuriosity into a sphere once given up to prudery and pruriency; (2) The effect of nakedness is beneficial on those of more mature age, also, in so far as it tends to cultivate the sense of beauty and to furnish the tonic and consoling influences of natural vigor and grace; (3) The custom of nakedness, in its inception at all events, has a dynamic psychological influence also on morals, an influence exerted in the substitution of a strenuous and positive morality for the merely negative and timid morality which has ruled in this sphere.

Perhaps there are not many adults who realize the intense and secret absorption of thought in the minds of many boys and some girls concerning the problem of the physical conformation of the other sex, and the time, patience and intellectual energy which they are willing to expend on the solution of this problem. This is mostly effected in secret, but not seldom the secret impulse manifests itself with a sudden violence which in the blind eyes of the law is reckoned as crime. A German lawyer, Dr. Werthauer, has lately stated that if there were a due degree of familiarity with the natural organs and functions of the opposite sex ninety per cent. of the indecent acts of youths with girl children would disappear for in most cases these are not assaults but merely the innocent though uncontrollable outcome of a repressed natural curiosity. It is quite true that not a few children boldly enlist each other's co-operation in the settlement of the question and resolve it to their mutual satisfaction. But even this is not altogether satisfactory, for the end is not attained openly and wholesomely with a due subordination of the specifically sexual, but with a consciousness of wrong doing and an exclusive attentiveness to the merely physical fact which tend directly to develop sexual excitement. When familiarity with the naked body of the other sex is gained openly and with no consciousness of indecorum in the course of work and of play, in exercise or gymnastics, in running or in bathing, from a child's earliest years, no un-

wholesome results accompanying the familiarity with the essential facts of physical conformation thus naturally acquired. The prurience and prudery, which have poisoned sexual life in the past, are alike rendered impossible.

Nakedness has, however, a hygienic value, as well as a spiritual significance, far beyond its influences in allaying the natural inquisitiveness of the young or acting as a preventive of morbid emotion. It is an inspiration to adults who have long outgrown any youthful curiosities. The vision of the essential and eternal human form, the nearest thing to us in all the world, with its vigor and its beauty and its grace, is one of the prime tonics of life. "The power of a woman's body," said James Hinton, "is no more bodily than the power of music is a power of atmospheric vibrations." It is more than all the beautiful and stimulating things of the world, than flowers, or stars or the sea. History and legend and myth reveal to us the sacred and awful influence of nakedness, for, as Stanley Hall says, nakedness has always been "a talisman of wondrous power with gods and men." How sorely men crave for the spectacle of the human body—even to-day after generations have inculcated the notion that it is an indecorous and even disgusting spectacle—is witnessed by the eagerness with which they seek after the spectacle of even its imperfect and meretricious forms, although these certainly possess a heady and stimulating quality which can never be found in the pathetic simplicity of naked beauty. It was another spectacle when the queens of ancient Madagascar at the annual Fandroom, or feast of the bath, laid aside their royal robes and in the presence of their subjects who crowded the palace courtyard, descended the marble steps to the bath in complete nakedness. When we make our conventions of clothing rigid we at once spread a feast for lust and deny ourselves one of the prime tonics of life. "I was feeling in despair and walking despondently along a Melbourne street," writes the Australian author of a yet unpublished autobiography, "when three children came running out of a lane and crossed the road in full daylight. The beauty and texture of their legs in the open air filled me with joy, so that I forgot all my troubles whilst looking at them. It was a bright revelation, an unexpected glimpse of paradise, and I have never ceased to thank the happy combination of shape, pure blood and fine skin of these poverty-stricken children, for the wind seemed to quicken their golden beauty and I retained the rosy vision of their natural young limbs, so much more divine than those always under cover. Another occasion when naked young limbs made me forget all my gloom and despondency was on my first visit to Adelaide. I came on a naked boy leaning on the railing near the Baths,

and the beauty of his face, torso, fair young limbs and exquisite feet filled me with joy and renewed hope. The tears came to my eyes and I said to myself 'While there is beauty in the world I will continue to struggle.'"

We must, as Bölsche declares (*loc. cit.*), accustom ourselves to gaze on the naked human body exactly as we gaze at a beautiful flower, not merely with the pity with which the doctor looks at the body, but with joy in its strength and health and beauty. For a flower, as Bölsche truly adds, is not merely "naked body", it is the most sacred region of the body, the sexual organs of the plant.

"For girls to dance naked", said Hinton, "is the only truly pure form of dancing, and in due time it must therefore come about. This is certain: girls will dance naked and men will be pure enough to gaze on them." It has already been so in Greece, he elsewhere remarks, as it is to-day in Japan (as more recently described by Stratz). It is nearly forty years since these prophetic words were written, but Hinton himself would probably have been surprised at the progress which has already been slowly (for all true progress must be slow) towards this goal. Even on the stage new and more natural traditions are beginning to prevail in Europe. It is not many years since an English actress regarded as a calumny the statement that she appeared on the stage barefoot, and brought an action for libel, winning substantial damages. Such a result would scarcely be possible to-day. The movement in which Isadora Duncan was a pioneer has led to a partial disuse among dancers of the offensive device of tights, and it is no longer considered indecorous to show many parts of the body which it was formerly usual to cover.

Attempts have here and there been quietly made to cultivate a certain amount of mutual nakedness as between the sexes on remote country excursions. It is significant to find a record of such an experiment in Ungewitter's *Die Nacktheit.* In this case a party of people, men and women, would regularly every Sunday seek remote spots in woods or meadows where they would settle down, picnic, and enjoy games. "They made themselves as comfortable as possible, the men laying aside their coats, waistcoats, boots and socks, the women their blouses, skirts, shoes and stockings. Gradually as the moral conception of nakedness developed in their minds, more and more clothing fell away, until the men wore nothing but bathing drawers and the women only their chemises. In this 'costume' games were carried out in common, and a regular camp life led. The ladies (some of whom were unmarried) would then lie in hammocks and we men on the grass, and the intercourse was delightful. We felt as members of one family, and

behaved accordingly. In an entirely natural and unembarrassed way we gave ourselves up entirely to the liberating feelings aroused by this light and air bath, and passed these splendid hours in joyous singing and dancing, in wantonly childish fashion, freed from the burden of a false civilization. It was of course necessary to seek spots as remote as possible from high-roads, for fear of being disturbed. At the same time we by no means failed in natural modesty and consideration towards one another. Children, who can be entirely naked, may be allowed to take part in such meetings of adults and will thus be brought up free from morbid prudery." (R. Ungewitter, *Die Nacktheit*, p. 58.)

No doubt it may be said that the ideal in this matter is the possibility of permitting complete nakedness. This may be admitted, and it is undoubtedly true that our rigid police regulations do much to artificially foster a concealment in this matter which is not based on any natural instinct. Dr. Shufeldt narrates in his delightful *Studies of the Human Form* that once in the course of a photographic expedition in the woods he came upon two boys, naked except for bathing drawers, engaged in getting water lilies from a pond. He found them a good subject for his camera, but they could not be induced to remove their drawers, by no means out of either modesty or mock-modesty, but simply because they feared they might possibly be caught and arrested. We have to recognize that at the present day the general popular sentiment is not yet sufficiently educated to allow of public disregard for the convention of covering the sexual centres, and all attempts to extend the bounds of nakedness must show a due regard for this requirement. As concerns women Valentin Lehr, of Freiburg in Breisgau has invented a costume (figured in Ungewitter's *Die Nacktheit*) which is suitable for either public water-baths or air-baths, because it meets the demand of those whose minimum requirement is that the chief sexual centres of the body should be covered in public, while it is otherwise fairly unobjectionable. It consists of two pieces, made of porous material, one covering the breasts with a band over the shoulders, and the other covering the abdomen below the navel and drawn between the legs. This minimal costume while neither ideal nor æsthetic, adequately covers the sexual regions of the body, while leaving the arms, waist, hips and legs entirely free.

There finally remains the moral aspect of nakedness. Although this has been emphasized by many during the past half century it is still unfamiliar to the majority. The human body can never be a little thing. The wise educator may see to it that boys and girls are brought up in a natural

and wholesome familiarity with each other, but a certain terror and beauty must always attach to the spectacle of the body, a mixed attraction and repulsion. Because it has this force it naturally calls out the virtue of those who take part in the spectacle, and makes impossible any soft compliance to emotion. Even if we admit that the spectacle of nakedness is a challenge to passion it is still a challenge that calls out the ennobling qualities of self-control. It is but a poor sort of virtue that lies in fleeing into the desert from things that we fear may have in them a temptation. We cannot dispense with passions if we would: reason, as Holbach said, is the art of choosing the right passions, and education the art of sowing and cultivating them in human hearts. The spectacle of nakedness has its moral value in teaching us to learn to enjoy what we do not possess, a lesson which is an essential part of the training for any kind of fine social life. The child has to learn to look at flowers and not pluck them; the man has to learn to look at a woman's beauty and not desire to possess it. The joyous conquest over that "erotic kleptomania," as Ellen Key has well said, reveals the blossoming of a fine civilization. We fancy the conquest is difficult, even impossibly difficult. But it is not so. This impulse, like other human impulses, tends under natural conditions to develop temperately and wholesomely. We artificially press a stupid and brutal hand on it, and it is driven into the two unnatural extremes of repression and license, one extreme as foul as the other.

To those who have been bred under bad conditions, it may indeed seem hopeless to attempt to rise to the level of the Greeks and the other finer tempered peoples of antiquity in realizing the moral, as well as the pedagogic, hygienic and æsthetic advantages[1] of admitting into life the spectacle of the naked human body. But unless we do we hopelessly fetter ourselves in our march along the road of civilization, we deprive ourselves at once of a source of moral strength and of

[1] I have not considered it in place here to emphasize the æsthetic influence of familiarity with nakedness. The most æsthetic nations (notably the Greeks and the Japanese) have been those that preserved a certain degree of familiarity with the naked body. "In all arts," Maeterlinck remarks, "civilized peoples have approached or departed from pure beauty according as they approached or departed from the habit of nakedness." Ungewitter insists on the advantage to the artist of being able to study the naked body in movement, and it may be worth mentioning that Fidus (Hugo Höppener), the German artist of to-day who has exerted great influence by his fresh, powerful and yet reverent delineation of the naked human form in all its varying aspects, attributes his inspiration and vision to the fact that as a pupil of Diefenbach he was accustomed with his companions to work naked in the solitudes outside Munich which they frequented (F. Enzensberger, "Fidus," *Deutsche Kultur*, Aug., 1906).

joyous inspiration. Just as Wesley once asked why the devil should have all the best tunes, so to-day men are beginning to ask why the human body, the most divine melody at its finest moments that creation has yielded, should be allowed to become the perquisite of those who lust for the obscene. And they are further convinced that by enlisting it on the side of purity and strength they are raising the most powerful of all bulwarks against the invasion of a vicious conception of life and the consequent degradation of sex. These are considerations which we cannot longer afford to neglect, however great the opposition they arouse among the unthinking.

"Folk are afraid of such things rousing the passions," remarks Edward Carpenter. "No doubt the things may act that way. But why, we may ask, should people be afraid of rousing passions which, after all, are the great driving forces of human life?" It is true, the same writer continues, our conventional moral formulæ are no longer strong enough to control passion adequately, and that we are generating steam in a boiler that is cankered with rust. "The cure is not to cut off the passions, or to be weakly afraid of them, but to find a new, sound, healthy engine of general morality and common sense within which they will work." (Edward Carpenter, *Albany Review*, Sept., 1907.)

So far as I am aware, however, it was James Hinton who chiefly sought to make clear the possibility of a positive morality on the basis of nakedness, beauty, and sexual influence, regarded as dynamic forces which when suppressed make for corruption and when wisely used serve to inspire and ennoble life. He worked out his thoughts on this matter in MSS., written from about 1870 to his death two years later, which, never having been prepared for publication, remain in a fragmentary state and have not been published. I quote a few brief characteristic passages: "Is not," he wrote, "the Hindu refusal to see a woman eating strangely like ours to see one naked? The real sensuality of the thought is visibly identical. . . . Suppose, because they are delicious to eat, pineapples were forbidden to be seen, except in pictures, and about that there was something dubious. Suppose no one might have sight of a pineapple unless he were rich enough to purchase one for his particular eating, the sight and the eating being so indissolubly joined. What lustfulness would surround them, what constant pruriency, what stealing! . . . Miss —— told us of her Syrian adventure and how she went into a wood-carver's shop and he would not look at her; and how she took up a tool and worked, till at last he looked, and they both burst out laughing. Will it not be even so with our looking at women altogether? There will come a *work*—and at last

we shall look up and both burst out laughing. . . . When men see truly what is amiss, and act with reason and forethought, in respect to the sexual relations, will they not insist on the enjoyment of women's beauty by youths, and from the earliest age that the first feeling may be of beauty? Will they not say, 'We must not allow the false purity, we must have the true? The false has been tried, and it is not good enough; the power purely to enjoy beauty must be gained, attempting to do with less is fatal! Every instructor of youth shall say: 'This beauty of woman, God's chief work of beauty, it is good for you to see it; it is a pleasure that serves good; all beauty serves it, and above all this, for its office is to make you pure. Come to it as you come to daily bread, or pure air, or the cleansing bath: this is pure to you if you be pure, it will aid you in your effort to be so. But if any of you are impure, and make of it the feeder of impurity, then you should be ashamed and pray; it is not for you our life can be ordered; it is for men and not for beasts.' This must come when men open their eyes, and act coolly and with reason and forethought, and not in mere panic, in respect to the sexual passion in its moral relations."

RETENTIVENESS IN CHILD AND ADULT[1]

By Cephas Guillet

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[1]I wish heartily to thank Mr. Hikozo Kakise for conducting the experiments with the Japanese names; also Dr. Sanford for helpful criticism.

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VI. CONCLUSIONS

I. BIRDS MEMORIZED BY CHILD

"Here 's another book for you from Santa Claus," I said to my two-year-old boy the day after Christmas. "Aw-w-w," he said, and chuckled all the way up stairs. It was a bird book, containing fifty-eight excellent, highly colored pictures of forty-eight different kinds of birds. I showed it to him and he said over all the names after me. He made me go over it all again, and this time he knew three of them, the rooster, blackbird and chickadee. The next day these proved to be the only ones retained. But the third day and nearly every succeeding day he had added new birds to his store, till the twenty-fourth, when he named all correctly. During this time he was actually at work (or rather at play, for we did not allow it to degenerate into lessons) for five and a half hours. This means that on the average each bird required 5.7 minutes, and each name 6.9 minutes to be learned. The canary, parrot, robin and thrush occurred twice in different sizes and positions, and there were two ducks, two grosbeaks, two wrens and three chickadees of different species.

The periods averaged nearly fourteen minutes, being a little

shorter at the beginning and at the end than at the middle of the experiment; at the beginning, because, knowing but few birds, he simply named most of them after me; toward the end, because he had become familiar enough with most of them to say them readily. We urged him very little and generally accepted his "I cawt say't," whether he meant I can't remember it or I can't repeat it. The longest periods were the 9th and 10th which each lasted twenty minutes. The length of time taken, of course, depended somewhat on his willingness, which in turn depended on his physical condition. On the 7th day, for example, at 10.45 A. M., after a run on the meadow, he felt good and showed great interest, saying all the words in about ten minutes; whereas the 9th day at 8.30 A. M., just after breakfast, he got tired and unwilling toward the end, refused to pronounce a half dozen of the last names and was at it twenty minutes. On the 19th day at 6 P. M., after supper, when he felt in particularly good spirits, he did not say "I can't" once and his reactions were generally very prompt. He knew most of the birds well and took but ten minutes to say them. The 24th day he required only six minutes to name all the birds without prompting and the day after he went through the book correctly in five minutes. He was never allowed to see the book except during these periods and we never suggested the birds to him, though naturally he sometimes recalled them to mind and spoke of them.

A week later he was found still to know all except one (redbird), but required ten minutes to repeat them, showing hesitation in recalling a few, and particularly redbird and yellowbird. Five days later, he still knew all but one, the same one, and said them in seven minutes. It is strange that the name forgotten had been learned on the sixth day and always thereafter said correctly. But he had been learning the colors lately and evidently was applying this imperfect knowledge to some of the birds instead of trusting his memory simply; for he called the redbird as well as the yellowbird "whitebird", while yellow warbler and redstart were recalled readily. Yellowbird was finally recalled but not redbird. He seems to have begun to analyze these two names but not the two former. I was perhaps responsible for this, for on the 21st day, the day after he had first said yellowbird correctly, when he hesitated at this bird, I asked "what color is that?" He replied "whitebird." (Thereafter he always said whitebird for yellowbird before he gave it correctly.) So now when I asked him what color the redbird was, pointing it out, he said first white, then blue, and finally red, but even then would not say redbird. On the last occasion above mentioned he had the same difficulty with the redbird as on the previous occasion, calling it

whitebird again, and he called the redstart simply "start," something he had never done before, though he had always had difficulty with it, not having learned it until the 15th day, and thereafter failing to say it again until the last day. It was indeed the only bird he missed on the day before the last. When asked what color the redbird was, he said red correctly this time, but then when asked what the bird was he said "redstart", though with some uncertainty. When now shown the redstart, he said redstart at once; but I could not get him to name redbird correctly. Three days later he called the redbird "whitebird" but told me the color correctly when asked. Yet when I said "What bird is it?" he replied "I don't know", although he named the redstart correctly at sight. Does this not illustrate the difficulty of trying to teach a child anything formally? Conscious effort would seem to be confusing. I have noticed in general that while the boy now uses the words red and blue correctly, yet when asked "What color is this?" he is quite likely to say blue for red and red for blue, white for black and black for white. The question seems to make him self-conscious, disconcerting him and affecting the correctness of his response. Of course later on when this association has been well established, so simple a question will not confuse him, but will not one more difficult, but relatively to his stage of development equally simple, be just as liable to disconcert?

The following table shows on each day the number of bird-names thus far learned, the number of birds that had been already learned, the new birds added that day, the birds forgotten that day (denoted by minus sign), and the total number of birds named that day:

1st day, three species (no birds previously known) blackbird, chickadee, rooster	3
2nd day, 3 species, (3 birds previously known)	3
3rd day, 8 species (3 birds previously known), cuckoo, duck (2) chickadee (2nd), kingfisher, crow, umbrella-bird	10
4th day, 10 species (10 birds previously known), parrot (2), owl, chickadee (3rd), —duck (2nd)	13
5th day, 15 species (14 birds previously known), albatross, grosbeak, doves, swallows, flamingoes,—rooster, parrot	17
6th day, 17 species (19 birds previously known), grosbeak (1st), macaw, redbird, —doves	21
7th day, 20 species (22 birds previously known), canaries, jackdaw, quail	25
8th day, 22 species (25 birds previously known), lark, nightingale, —canaries, doves	25
9th day, 25 species (27 birds previously known), hawk, lovebirds, martin, swallow, —canaries, doves, lark, nightingale	27

10th day, 25 species (31 birds previously known), —canaries, doves, hawk, lark, nightingale 26
11th day, 27 species (31 birds previously known), canary, ibis, pigeon, —canaries, doves, hawk, jackdaw, nightingale 29
12th day, 30 species (34 birds previously known), goldfinch, hummingbird, thrush (2), hawk 37
13th day, 36 species (38 birds previously known), jay, nuthatch, ostrich, robin (1st), vireo, vulture, —doves, hummingbirds, goldfinch 41
14th day, 38 species (44 birds previously known), stork, wren (2nd), —doves, vulture 44
15th day, 40 species (46 birds previously known), redstart, heron, —vulture 47
16th day, 40 species (48 birds previously known), hawk, hummingbirds, redstart, wren (2nd) 44
17th day, 41 species (48 birds previously known), yellow warbler, —doves, hawk, redstart, thrush (2nd), vulture 44
18th day, 42 species (49 birds previously known), cassowary, robin (2nd), —hawk, redstart, yellow warbler 48
19th day, 46 species (51 birds previously known), eagle, wren, magpie, partridge, yellowbird, —hawk, redstart, yellow warbler 52
20th day, 46 species (56 birds previously known), —hawk, redstart, yellow warbler, robin (2nd) 52
21st day, 47 species (56 birds previously known), secretarybird, —redstart, robin (2nd) 55
22nd day, 48 species (57 birds previously known), stormy petrel, —redstart, yellowbird 56
23rd day, 48 species (58 birds previously known), —redstart 57
24th day, 48 species (58 birds previously known) 58

Between the 17th and 18th days a day intervened upon which no trial was made. He was on a visit to a neighboring city that day and the two preceding, and it was found that the presence of strangers and strange surroundings somewhat excited him and interfered with the naïveté of his behavior. This was plainly mirrored in the fact that on each of these days he named only 44 birds, although the previous day he had named 47 at home and the day after he returned he named 48. There was only one other instance of his losing ground, namely the 10th day, when he named one less than the previous day.

It will be seen that the child added on an average about 2½ birds per day to his store, and that the number of *new birds* added each day fluctuated between none (3 times) and seven (once). It cannot be said that there was a slowing down of

the speed of acquisition in the latter part of the time. There were 3 apexes of acquisition, namely on the 3rd, 12th, and 19th days (7, 8, and 5 net *additional* birds as compared with the net number recalled on the previous day). After each of these the curve fell with considerable regularity until the next almost abrupt rise. It reached its lowest points, in fact, on the 8th and 10th, the 16th and 17th and the 20th, 22nd, 23rd, 24th. The curve of acquisition of new bird-*names* was much the same, reaching its highest point, however (6), one day later than the other curve. The acquisition was slow at the end, probably because there were few new birds left to learn and these very difficult (secretary-bird and stormy petrel).

Some names were particularly easy of acquirement, others particularly hard. Of the easy ones rooster was a word already known and the bird was known; the word blackbird had been heard, but had not been associated with a bird; chickadee proved an attractive sound, as it was learned immediately, though never heard before. Cuckoo was very nearly his way of saying cookie (coocoo), so he possessed the word and only needed to get a new association for it, which proved easy; umbrella-bird was easily learned for a similar reason, the oddly-shaped crest of this big bird doubtless helping. Duck, crow, and kingfisher had all been heard and used of the live birds, the last two the previous summer, and the first frequently since then. None of these birds, once learned, ever failed to be recalled, except duck and rooster once, and this last was probably due to willfulness or playfulness, a factor that must be reckoned with in studying children. Other birds that proved easy to learn and recall are owl, parrot, albatross, grosbeak, swallows, flamingoes, macaw, and redbird, all of which are in the first half of the book except the last two.

On the ninth day when he was still unable to recall lark, I substituted the name sparrow because the English lark as pictured looks much like a sparrow, a more useful word for him. He would not pronounce it, however, saying "I cawt". The next day he again refused, but the following day he repeated "payo". Yet the day after, when asked to name it, he recalled its original name, saying "lock". This is an interesting case of the persistence of an association. He failed, however, to recall it the two following days, but never thereafter.

Words particularly hard to learn were redstart, stormy petrel, secretary-bird, magpie, hawk, partridge, yellowbird, eagle, wren (1st), robin (2nd), cassowary, doves and yellow warbler, all of which were in the second half of the book except eagle, hawk, doves and wren. I called the second wren "Jenny Wren" and the first robin "Robin Redbreast", while

the others were named simply wren and robin. The longer name proved considerably easier to learn and remember in both cases. I did not call the second wren, which was our common house wren, "Jenny Wren" until the seventh day. He always kept these two names in their first association, but, though he did this at first in the case of the two robins, he finally applied "Rob Rebest" to both. The fact of the hawk and eagle occurring together on the same page and resembling each other retarded their acquisition. The same was true of the English robin and redstart and the martin and magpie. In the last case the names also are somewhat similar, especially as he called the martin "mockin". When, of a word that he could not recall, the first letter or syllable was sounded, he could usually finish it, but such cases were not counted. We frequently found that a little encouragement was helpful. When he would say "I can't", our "O yes you can, that's a—" would often bring the name. A coaxing request also several times induced successful effort. The "I can'ts" were much more numerous in the second half of the book.

Adding together the number of days each word took to be learned and dividing by the number of birds, I find that the first 29 birds in the book took on an average 7.6 days to learn, and the last 29 birds 12.2 days, or, taking the bird-names only, the first 24 bird-names required an average of 7.8 days, the last of 12.1 days each, showing clearly the effects of fatigue in retarding the acquisition of the associations occurring latest. We always began at the beginning of the book except on one day, the 12th, when we began at the end. On this day the child was remarkably successful, making the largest gain of any day during the experiment. The previous day he could recall 29 names, this day he recalled 37.

He is very fond of seeing me draw cats and mice and frequently says "Mate a kitty, dado". On the ninth day of our experiment he began to ask me to "mate a umbella-bi(r)d", "Mate a tush-bird", "Mate a nightingale singin away", "Mate a gosbeat-a pawt". It is curious that though he had never yet succeeded in recalling the name "thrush" during the regular lessons, yet he here used the word and in an original form (tush bird). He had kept up his interest in this exercise for 23 minutes, when we were interrupted. On six other occasions during the course of the experiment he called for the drawing of certain birds and this no doubt helped to fix some of names in his mind. Those asked for besides the ones above mentioned were in order kingfisher (12th day), chickadee and owl (the day of no trial), vulture, hawk, macaw, nuthatch (18th day), duck (21st), blackbird, ostrich (22nd). All these were learned early and retained well except "hawk". The umbrella bird

and grosbeak were called for on five of the seven occasions, none of the others more than three times. During the week that he was not allowed to see the book, he asked once for me to draw the ibis, eagle, and hawk. I, of course, drew only those that were asked for. One day he commanded "Mate a caw, mate a caw, mate a *me*-caw, a *me*-caw". When I drew the macaw, he said "At's a hawk". So I determined to take a few more lessons from the book before I should be called on again by this critic of two years and three months!

He would sometimes use bird names in his prattle and his sing-song. This I observed six times during the period when he was acquiring them, the names so used being umbrella-bird, flamingo, chickadee, and grosbeak twice, and macaw, warbler, rooster and jenny wren once. He once said "I'm a grosbeak, I'm a macaw, I'm a chickadee," and one night he half awoke from sleep and asked dreamily, "Wha's the hummin-bi(r)ds?" Three days after he had learned all, as he lay in bed after waking, he said over to himself fourteen of the names. The same day, pointing first to me and then to a little friend, he said, "At a pawt and dis a gosbeak." Then putting his hand on my nose, he said, "At's a beak," then he added "a hawk." The next day, turning his back towards me, he said, "Look at my tail." These are rather remarkable applications of his bird-lore. He added on the last occasion, "I'm a jay; I'm two love-birds." There are two love-birds in his book—"kissin togedda," as he expressed it in his original way the day before. We had told him they were kissing each other. Two days later he asked his mother, "What does the redstart say?" His mother passed the question on to me, but, before I could answer, he said, "see me, see me, see me;" I had told him that fully a week before.

The following proved to be difficult where not impossible sounds throughout the experiment: the simple consonants r, v, y, th; the combinations represented by kw, sw, -ing, st, sp, -tsh, j, -dg, -dsh, wh; and many compounds of l and r with other consonants (lb, bl, fl, ld, sl, kl, tr, gr, str, kr, thr, br, rk, rd, rt, rbl). The following were fairly mastered during the experiment: lb, kw, j, x, -dg, -tsh, st, th, -vs, ch. The child prefers to accent the first syllable, as fam'ingoes, me'caw, no doubt by analogy with most English words. The child used b for v, f for sw, d for j, p for sp, t for st, sh for tsh, w for r, f or t or later th for thr, and initial y was omitted.

On Feb. 12th, for nearly half an hour, beginning at 1.45 P. M., I had the child name the birds whose heads I showed him, covering all the rest of the bird, and indeed of the book, with papers in which I had cut holes of different sizes to show the heads. I did not show him two birds consecutively on the

same page but skipped about through the book. He would run away after each and come at my call with great glee thinking it a fine game. After I had tested him on 23 birds, fearing fatigue, I desisted and finished the remaining 32 in the evening in 50 minutes. He named all correctly but 10, namely vulture (called swallow), jackdaw (magpie), thrush (dove), redbird (yellowbird), nightingale (canary), yellow warbler (yellow bird), lark (thrush), swallow (martin), ostrich (partridge), cuckoo. I tested his mother in the same way and she missed the following fifteen: jackdaw (called blackbird and stormy petrel), magpie (blackbird, crow), blackbird (crow), redstart, yellow warbler (thrush), lark (nightingale), dove (cuckoo), swallow 1st and 2nd, chickadee, duck, martin (swallow), cuckoo, partridge (pigeon), chickadee, 2nd. The duck was an English grebe. The child's responses were almost invariably prompt, in fact instantaneous. For the robin he first said "canady-bird," but corrected himself.

On Feb. 14th and 15th I tested his ability to name the birds minus their heads and all surroundings. Of the 58 birds he was able to name correctly all but two, the ostrich and the first grosbeak (called pigeon). The ostrich was called partridge, just as when only the head was shown, but I think that in both cases this was purely a confusion of the names, since he named it correctly next day when a similar trial was made. However, he also named the grosbeak correctly next day. This bird and the pigeon are much the same size and form in the book, but the pigeon is pink while the grosbeak is gray. The head evidently helped him most with the grosbeak, on account of the large red beak, for, when I excluded everything but the beak, he said "grosbeak," after exclaiming, "Any (*i. e.*, no) head on it!" which he sometimes did of birds without heads. It is interesting to note that when I showed him the pigeon without its head the day after he had called the grosbeak pigeon, he called the pigeon grosbeak at first and then pigeon. From this it would appear that form is more noted by this child than color. The other headless birds that were only named correctly at the second guess were redbird (called yellowbird), lark (humming-bird), hawk (grosbeak), vireo (wren), and a flying canary (sparrow). These mistakes also suggest that color is of minor importance to this child at present as a distinguishing mark. While he named the headless jackdaw correctly the first day, yet the next day, when I showed it to him again, he first called it magpie and then jackdaw. Now the magpie and jackdaw resemble each other very closely except in color. Again, the heads of these birds are of exactly the same color (a blue black), yet he only confused them once out of four times, being shown both heads

on two successive days. The jackdaw's head is a little larger and is turned in the opposite direction from the magpie's, which are the only clues he could have, seeing that the bills are alike as well as the shape of the head. The eyes are a little different. I carefully covered the spoon the magpie carried in his bill. That action is of marked interest to this child is shown by his calling the heads of the first robin and the nightingale canary. All these birds are represented as singing with their heads turned up and their beaks open. It would seem as though action played a more important part than color in his perceptions. Similarly the singing lark's head was called thrush, which is also represented as singing; his mother called it nightingale. Again the headless lark was called hummingbird. Here there is no resemblance except in the wings being outspread in flight in both. The color is quite different (mauve and brown). Moreover the headless flying canary was called "sparrow," by which he meant lark. One questions whether children note distinctions of color consciously before their attention is called to them and the names given. We know how many things escape ourselves until we have been trained to observe them, for instance, the many curious and brilliantly-colored mushrooms in the woods. Even as I write, this child, now two years and a half, though he uses correctly black, red, white, yellow and generally blue, yet calls all his light-colored marbles (yellow, light brown, etc.) white, and those of dark color (blue, dark red, purple, etc.) black. The child needs to know the form of things more than their color, and color is given to him through only one sense while form is conveyed to him through not only sight but particularly the very emphatic sense of touch. Color is only one factor in the building up of the perception of form and would seem not to be noted as such and for its own sake at first. His confusion of redbird and yellowbird, both when the head alone of the redbird was shown and when it was shown headless, may be only a confusion of color names. The head of the yellow warbler was called yellowbird, but no other mistake was made with these two birds. The vulture is evidently known by the great size of its body (it is the largest bird but one in the book), for its head was named swallow one day and thrush the next. The second thrush's head, which is not in the attitude of singing, he called first dove with uncertainty and then said "eye," "beak" (pointing to them), then "I don't know." But, being urged to say, he guessed "swallow." These are the only bad mistakes he made. It is not remarkable that he should have called the head of the second swallow martin. The surprising thing is that he should have named the head of the first swallow and of the martin correctly, and that he should not have

confused the heads of the eagle and the hawk (a falcon). In the case of the martin and swallow I think color must have played a part, as I showed him three different swallows' heads, one of which is turned in the same direction as the martin's and is in every respect similar except in color (being brown, while the martin's is a blue-black), and in the martin's head being slightly larger. The head of one of the other swallows is of a different color again, a dark purple with reddish throat, while the martin and the other two swallows have a white throat.

His calling the canary sparrow is a point of much interest for a different reason. When I showed him the headless flying canary, he hesitated a little while and then said "spayo." I said, "No that 's not a sparrow, and then he said "canady-bird." He had not heard the word sparrow since the three days upon which I had given him that word for the flying lark, and he had only repeated it once after me, namely on the last of the three days when he said "payo." Now forty-five days after, he recalled this word and applied it to a bird flying like the lark. The sound "sparrow," therefore, had remained reproducible, associated with the visual image of a bird flying (during all this time), and was now for the first time called forth in connection with the visual stimulus of a flying bird. With the head the bird would have been called canary as usual but the headless canary did not at once suggest the name canary, and hence there was a chance for the other association to appear. That he should have called it sparrow rather than lark is possibly due to the fact that "lark" also had through frequent repetition got connected more closely with the visual image of the whole bird.

In order to see how much further the child had carried his unconscious analysis (if I may be allowed the expression), I tested him during the next few days in recognizing the birds when seeing only the bill or, if the bill was not then recognized, extending to and then beyond the eye till the whole head, or more if necessary, was uncovered. I also showed crests alone where present, and, in the case of the owl, the eyes alone. I found that the quail, rooster, and umbrella-bird were recognized by the crest, and the owl by its eyes. The rooster was not known by the bill alone, but the quail was; when shown the umbrella-bird's bill, he said first eagle, then at once umbrella-bird. The following birds were recognized when only the bill was shown: both grosbeaks (one with red, the other with brown bill), flamingo, ibis, macaw, first parrot (large red bill), first canary (not singing), both ducks, pigeon. The following were recognized when a little bit of the head was seen with the bill but not including the eye: first thrush (sing-

ing), first and third chickadees, stormy petrel, lovebird, martin, nuthatch, first swallow, first robin (not singing), goldfinch, dove, yellow warbler, vireo. Not recognizing them at once he would say, "Lill bit more, dado." The following were recognized as soon as the eye appeared: stork, blackbird, albatross, second canary (singing), hawk, second robin (singing), kingfisher, hummingbird, yellowbird, jay, partridge (first said pigeon), eagle, second parrot, second chickadee, crow, second thrush (said "I can't see it, make his eye"), nightingale (said "thrush" for bill only), redstart, secretary-bird. The second "chickadee," a long-tailed tit, has a small black bill much like the martin's but more hooked; before he saw the eye he called it hawk and then martin. He said magpie for jackdaw; but when shown the latter another day, he said magpie when only the bill was shown, but jackdaw as soon as the eye appeared. Of the following the whole head had to be shown before they were recognized: cassowary, heron (fish excluded), lark, magpie (spoon excluded), first wren, ostrich, redbird (but called yellowbird as usual), vulture. A little of the body had to be shown with the head for the recognition of the cuckoo and the second wren; and on two different days the second swallow was called martin until the whole bird was seen. There is evidence of considerable redintegration here, or such an association of the whole with the various parts of which it is composed, that the part, and often a very inconsiderable part, at once suggests the whole, doubtless in a very mechanical and unconscious way. Three birds were known by their crests, one by its eyes, and a dozen by their bills; in thirteen others a small part of the head only needed to be added to the bill; twenty others were known when the eye was added. This leaves eleven birds, eight of which were known when the whole head was seen and two more when the wing-shoulders were added. This leaves only one bird that had to be seen entire to be known, and this bird was confused with one which resembled it closely in every way but in color, in which they differed markedly. Both were flying, though in opposite directions.

While he was learning the birds, attention was scarcely ever called to any part, but he occasionally pointed to and named the tail and eye, and later the beak and legs, as "Look at a eye!" "Little tail stickin' up", "Long beak!" "Look at a big legs!" He would sometimes refer to their actions as "singin' away, he's flyin', sittin' on fence, he's climbin' a pole", the last two original. He noted surrounding objects as the duck's and dove's eggs, the snake in the secretary-bird's bill, and the fishes held by kingfisher and heron.

He was tested with the first twenty birds in the book by

showing him the tail or feet in the same manner that the head had been shown. The following fourteen birds were known from the tail alone: wren and ibis (tip only), rooster and jackdaw (half of tail), albatross, blackbird, chickadee, swallow, dove, cuckoo, both ducks, goldfinch, jay. He knew the flamingo by its feet, the cassowary by its feet and legs, the eagle, hawk, and canary by the feet and tail, the grosbeak by the tail and wing, and the hummingbird by its wing, feet and tail.

I also had him guess the birds from their association with the others on the page. On two pages there was but a single bird, but on each of the other thirteen pages there were on the average about four birds. I tested him on only one bird a day on each page for four days, covering the bird up completely along with its immediate surroundings and leaving the rest of the page uncovered. The only way he could guess the name of the bird was by its association in his mind with the other birds on that page and by its position on the page. During the four days he was tested with fifty-one birds, of which he guessed twenty-six correctly and twenty-five wrong. The exact numbers each day were 6 right and 7 wrong, 6 right and 7 wrong, 8 right and 5 wrong, 6 right and 6 wrong.

II. Birds Memorized by Adult

To compare my memory for new oral verbal associations with the child's, I had myself taught similarly the Japanese names for the same birds. Mr. Hikozo Kakise kindly consented to conduct this experiment with me. It came out that I required thirteen days to learn the names, or about half as many as the child. But as my method of learning the names differed from the child's, in that I consciously strove to recall the names and to add as many as possible each time, I took much more time each day than did the child. In fact the total amount of time spent by me in the experiment was 4 hr. 50 min., or only 40 min. less than the child used. It should be mentioned, however, that some time was consumed each day in note-taking. I used an average of 22 min. a day, and one day occupied 40 min. On the 13th day, when I finally succeeded in saying all the names correctly, I required nine minutes to do it; while the child on his successful day required but six minutes. The day after, however ("14th day"), I named the birds in exactly the some time as the child did the day after his first success, namely in five minutes. A week later ("15th day"), I required a little over nine minutes to name all the birds, finding three particularly hard to recall, namely, hikuidori, yamasuzume, and umisuzume. Six days thereafter ("16th day") I required eight minutes to name the birds, finding difficulty only with ouiwatori and uzura. After a further interval of six weeks

("17th day"), however, I was found to have lost nine of the words, two absolutely (hikuidori and kawarahiba), four less completely (akazuru, hibari, hebikuidori, and kasadori), and three I got nearly right (hototogis, uzura, saezuridori). Five others were hard to recall (bakadori, kuro-tsugumi, aka-garas, kasasagi, wase). Moreover, I used thirty-five minutes, or more than four times as much time as six weeks previous. It will be seen that my experience tallied herein very closely with the child's. The child required five minutes more to recall the words after a week's interval, and I a little over four minutes. He did it three minutes better five days later and I a little over one minute better six days later.

It must be taken into consideration that the birds were all familiar to me and so stood out distinct from one another, ready to have a new name attached to them, while they were nearly all quite new to the child. Also my power of articulation is better than the child's. On the other hand, I had no knowledge of Japanese except what every one picks up in his reading, while the child already possessed a large vocabulary of English words, which doubtless had some suggestive value, especially as it comprised some of the words occurring in the bird names; namely, bird, red, black, crow, umbrella, kingfisher, "coocoo", duck, rooster, Jack, love, caw, night, nut. My knowledge of several foreign languages, however, doubtless offset this. Again the child, as we have seen, rather frequently recalled bird names to mind and used them, while only very rarely did any Japanese name recur to my mind and it was never dwelt upon.

In examining Mr. Kakise's notes of my method of recalling, so far as he could gather it, and supplementing his records from my memory, I find that the matter may be resolved into the following kinds of reaction:

a. The mixing of the name wanted with some other.

b. The substitution of another name for the one wanted.

c. Some of the letters and possibly the general character of the word recalled, but generally not visualized; if visualized, in my handwriting.

d. An impression of the general character of the word,—the number of syllables or the length of the word, the difficulty or lack of familiarity, the heaviness or lightness of sound. Waving the hand to indicate the rhythm sometimes helped me recall a word.

e. Making a "bluff" at it and succeeding or nearly without any visual or auditory image to guide me, the vocal organs my sole guide; no consciousness till the word is struck.

f. Method of trial and error: using the vocal organs directed by my ear till, by a process of elimination of what did not

sound right, I finally struck the right word, or what seemed to me right or nearly so. Sometimes I began by a gutteral trilling.

g. A feeling of familiarity, an inkling, a foreshadowing, as if the word or sound were approaching and projecting a vague image of itself before it.

h. (1) A feeling that I am nearly right, but not satisfied.
(2) A certainty that I am wrong.

i. Recalling one of the parts of a compound word.

j. Association with previous knowledge.
1. Of other languages.
2. Of Japanese.
3. Association of character of word with character of animal.

k. Recognition of word when prompted.

l. Ability to finish word begun for me.

m. Recalling the first part or the last part more or less correctly, and then repeating it till it possibly called forth the rest by association.

n. Elimination of the wrong word by being assured it was wrong; and then the line of least resistance to the effort was along the path made by the true word.

o. Uncertainty whether right after pronouncing the word correctly; failure to recognize the word completely after I say it.

p. Mind a perfect blank, incapable even of making a "bluff" at the word.

q. Persistence of an error once made.

The method of trial and error (f) was the most characteristic, and there were several cases of blind attack or "bluff" (e), when I succeeded by the sheer effort of speech without any conscious guidance. There were also many cases of the strange feeling of familiarity without further content or possibility of recalling anything (g), as well as of an impression as it were of the ghost of the word, its general character, without any image, visual, auditory, or motor, of the letters or sounds, but only a vague feeling of the rhythm of the word or some other quality (d). Some use was made of associations as memory hooks (j); on the other hand, there were many cases of interference (a and b).

A slightly wrong pronunciation was sometimes accepted as correct, the error not being detected. Had I been able to see the word or hear it spelled, this could hardly have occurred. I only discovered it when shown the words after the experiment was over.

Beginning with the second day the number of bird-names recalled by me up to and including the day in question, and

the net number of bird-names actually recalled that day were as follows :

2, 7, 11, 14, 22, 26, 34, 41, 43, 45, 47, 48;
2, 7, 11, 14, 22, 25, 32, 40, 41, 42, 46, 48,

It is perhaps worthy of remark how few words were lost once they had been recalled without aid. I made 10 misses, my little boy 47, with the birds. The curve of acquisition both for new names and for net number of additional names each day has three distinct apexes at the 3rd, 6th, and 8-9th days (5, 8 and 8 names respectively), with strong dips at the beginning, and end and between; but there is another for net additional names at the 12th day (4 names). This makes a very regular curve with intervals of 2 days between the apexes.

Fatigue induced by the experiment must have played a very small part in my case, for the first 24 names in order as they occurred in the book required on an average 6.7 days each, the last 24 each 7.1 days to learn or a difference of less than half a day in favor of the first half of the book. In my experiment with the mammals, to be described later, I began on alternate days at the beginning and the end of the book. In this case the first 26 required each on an average 7.2 days and the last 26 required 6.1 days each to memorize.

III. Mammals Memorized by Child

On the 26th of February, in his 30th month, I began to show my little boy a mammal book in the same way that I had shown him his bird book. This was Schubert's *Naturgeschichte der Säugetiere*". From it I selected 52 animals, all different, of which he was to be given the names, half English and the remainder foreign, equally divided between French and German. I took care that as nearly as possible the words in each language should be of the same length. Of one syllabled words there were 4 English, 3 French and 2 German; of two syllabled words there were 14 English, 6 French and 9 German; of three syllabled words 7 English, 4 French and 2 German; and there was 1 English word of 4 syllables. Thus the 26 English words contained 57 syllables and the 13 French words 27 syllables, and the 13 German words 26 or 110 syllables in all. I also so distributed the names that 12 English, 7 French and 7 German words occurred in the first half of the book and 14 English, 6 French and 6 German in the second half.

Mindful of the influence of fatigue in retarding the learning of the names of the birds in the last half of the book, I began at the beginning and at the end of the mammal book on alternate days. The result showed very clearly how essential this change of

method was to the experiment, and at the same time furnished remarkable evidence of the immense importance to pedagogy of the question of fatigue. During the first seven days, the child learned to name correctly 15 animals, and although they were equally distributed between the first and second halves of the book (8 in the first half and 7 in the last), yet every one was correctly named for the first time in the first half shown him. Again, the total number of correct recollections during the seven days was 38, of which 20 were names of mammals in the first half of the book and 18 of mammals in the second half. Yet the number that occurred in the first half for each day respectively was nearly twice as great as that of the names occurring in the second half for the day, or 24 and 14 respectively. Not only did the child not learn any new words when fatigued but he forgot some he had already learned. For example, the skunk which is the first animal on the list, was named correctly on the 5th day, while on the following day when he began at the end of the list, he could not recall it clearly, saying "hunks" instead of skunk. This word "hunks", whose inception was evidently due to fatigue, persisted through nearly the whole of the experiment, although corrected each day. Sea-lion and narwhale which occur in the second half of the book and were named correctly for the first time on the 6th day, when we began at the end of the book, could not be recalled on the next or 7th day, when they occurred in the second half as taken up on that day. To attempt therefore to teach a child anything when fatigued is not only futile as regards new acquirement but is even destructive of progress already attained. Moreover the effects of fatigue are subtle and not always easily or readily discernible in the behavior or appearance of the child. It was not until the 7th day that I noticed signs of fatigue in the child towards the close of the experiment, namely holding his head with his hands, and tripping and falling as he ran away. However, I divided the words into two equal sections and gave him them thereafter at different times of the day, while still continuing the alternation in order. The result was a marked change. The number of words repeated in the first half of the experiment and the second half was about the same, even a few more in the second (255 and 262). The number of new words that came in in the second half for each day was twice as great as in the first half (25 to 12). Two periods in the day, therefore, of 10 minutes each proved not to be too much for the child. Let us hope that the suffering that this child, healthy as he is, must have undergone through fatigue in the earlier part of the experiment will prove to have been vicarious, and by its suggestive

value help to lift the burden that weighs so heavily upon childhood throughout our land in school and shop and factory.

The child required 21 days to learn the mammals, less by 3 days than he took to learn the birds. But as the time spent each day was longer, being on an average 19 minutes, and about six hours in all, there was about an hour more time given to the mammals. It should be remembered that there were four more *names* of mammals than of birds, but six fewer mammals. At times his attention was very flitting. Once he ran to his mother after each animal to tell her the name. On that and one other day he consumed 30 minutes which was the longest time spent over the book on any one day. The shortest time was 12 minutes and occurred four times, the 2nd, 5th, 19th and 21st days. It should also be mentioned that this experiment was only conducted during five days of the week, Saturdays and Sundays being omitted, except the 21st day which was a Saturday. The experiment thus lasted 4 weeks. Each mammal required on an average 7.6 minutes to be learned, as compared with 6.5 for the birds. It should be remembered that half the mammal-names were foreign, and hence, as will be seen, harder to learn.

It seemed to us throughout that he took less interest in the mammals than in the birds. This might be accounted for by the brilliant colors of the birds and especially their greater naturalness and activity. The mammals are represented in quite wooden postures. In books prepared for children it would be better to have a few good illustrations, representing the animals at work and play, than a great number of reproductions of museum specimens. My boy was constantly on the lookout for activity, and in its almost total absence, he would try to supply it, making use of every hint. For example, many of the pictures in the mammal book contained representations of water; so the child would frequently say "He 's drinkin' ", though no animal was represented as in the act of drinking. "He 's goin' to catch it", he said, seeing a duck in the water with mammals on the shore, but no mammal was represented as trying to catch anything. Also he said that the raccoon was going to jump on the bear though the illustrator was innocent of any such intention. Perfect decorum was observed by all the animals, who evidently had sat for their pictures. He was particularly interested in the tails and eyes of the mammals and in their open mouths. "He goin' to bite me," (putting his hand on the open jaws) "he 's got some eye" (hedgehog), "where his eye? there 't is" (porcupine) "there 's his eye" (whale), "he has some tail" "look at a horns." He noticed the mole had no eye visible "any eye!" (no eye). The keenest observation, perhaps, was

his remark on seeing the forlorn looking donkey: "he 's cwyin' ".

The foreign words were introduced in order to test the child's relative ability to learn foreign and English words. The child's playful prattle contains an exceeding variety of sounds, many of which are thought to be lost through the growing specialization imposed by the necessity of learning and using exclusively a single definite set of sounds, known as English or French, etc. It seemed to me of interest to discover how far such specialization had already proceeded in a child so young.

The following table shows for each day the number of mammals so far learned, the new ones added that day, those forgotten that day, and the total number named that day. Those forgotten are preceded by a minus sign.

2nd day, 1 mammal, camel	1
3d day, 3 mammals, spitzmaus, anteater	3
4th day, 4 mammals, goat	4
5th day, 7 mammals, skunk, zobel, porcupine	7
6th day, 12 mammals, lama, gazelle, giraffe, sea-lion, narwhale, —skunk	11
7th day, 15 mammals, wiesel, jackal, schnabeltier,—skunk sea-lion, narwhale	12
8th day, 18 mammals, ours, dachs, zébu, —skunk, zobel	16
9th day, 26 mammals, hedgehog, guinea-pig, armadillo, wild boar, tapir, chamois, seal, wal, —skunk	25
10th day, 32 mammals, maulwurf, opossum, kangaroo, muskrat, biber, taureau, —skunk	31
11th day, 33 mammals, chevrotain, —skunk, guinea pig, kangaroo, wild boar, taureau, narwhale	27
12th day, 37 mammals, manis, rhinoceros, zebra, morse, —skunk, wild boar, chevrotain, taureau	33
13th day, 38 mammals, ferret, —skunk, rhinoceros, wild boar, chevrotain, zebra, morse	32
14th day, 39 mammals, musk-ox, —skunk, rhinoceros, zebra, sea-lion	35
15th day, 43 mammals, flusspferdt, esel, seebär, manatee, —skunk, musk-ox, zebra	40
16th day, 44 mammals, dauphin, —skunk, flusspferdt, chevrotain, esel	40
17 day, 46 mammals, loutre, moose, —skunk, flusspferdt, esel, seebär	42
18th day, 48 mammals, raton, renntier,—moose	47
19th day, 49 mammals, edelhirsch	49
20th day, 52 mammals, renard, volverenne, paresseux, —edelhirsch	51
21st day, 52 mammals	52

The aggregate number of days the child said correctly the 26 English words was 318, the 13 French words 94 days, and the 13 German words 138 days. French words are, therefore, if we can draw any inference from this test, nearly twice as hard as English, and German words not one quarter harder than English; or to be more exact, French is 59.1 per cent. as easy as English, and German 83 per cent. as easy. Thus German is 23-9 per cent. easier for this child than French. As he is partly of French descent and not at all of German, it would appear that heredity must play but a small part in the acquirement of language, and the onus of proof rests with those who hold that it has any direct influence. To a child whose mother tongue is English, it is not surprising that German, a Teutonic language like our own, should have proved easier than French, as that language much resembles our own in its pronunciation and particularly in its accentuation. The German way of pronouncing *sp* was no harder for this child than the English and the *z* and *schn* proved easy; the *pf*, however, he never mastered. *Flusspferdt* and *Edelhirsch* proved very difficult, and *Maulwurf* rather difficult. On the 8th day, by watching my lips, he was able to pronounce *Wiesel* without substituting a b for the w, but he did not pronounce it correctly of his own motion till the last two days. He had the same difficulty with *Wal* and *volverenne*. One of his chief sources of trouble with French, besides the accentuation or lack of strong stress, was the frequent occurrence of the letter *r*. It happened that every French word but three contained this letter. The French words in order of ease beginning with the easiest were *ours*, *zébu*, *chamois*, *armadillo*, *taureau*, *morse*, *dauphin*, *loutre*, *raton*, *renard*, *volverenne*, and *chevrotain*. The last two he never pronounced correctly. I had to be satisfied with chemotain and volvenne at the best. *Volverenne* was bolwenne and bolvenne until the 20th day when he recalled it for the first time and called it volvenne. For *paresseux* he said cacasseux on the ninth day, and thereafter when he recalled this word he always so called it until the last day when he corrected himself and added "paresseux." His pronunciation of the so-called nasals is interesting. These sounds evidently puzzled him. At first he said daupha and wato; but he seemed to feel there was something lacking in his pronunciation and to supply the lack he added the syllable nna and said dauphanna and dauphinna (8th day to 20th) and ratonna (13th day to 18th inclusive) and sometimes temotan and chemotanna. It is remarkable that he should have supplied an n, as I did not suggest such a letter except in so far as a proper pronunciation of the nasal *in* and *on* would do so. He had no difficulty with any other vowels, however, in either language, even the

French *u*. For chamois he persisted in saying "chambwa," like wha in wharf. This was perhaps his way of mimicking the foreign accent. English words that proved hard to pronounce were ferret, kangaroo, giraffe, porcupine, zebra, and especially rhinoceros, which he never could say better than wynoceos and frequently the *n* was left out as well as the r's. Porcupine was always portupine, giraffe dewaff. Altogether I am inclined to think that those who speak of the infinite variety of the child's instinctive speech in the babbling stage are possibly correct as regards vowels but probably mistaken as regards consonants. In any case it seems as though if one wished to take advantage of the "nascent period" for speech in the acquirement of a foreign language, one would have to begin it at birth, where many other nascent periods lie. The first sound my own child made (namely, in crying) was kazthá, the second consonant lying somewhere between a *z* and a *th*, in fact just like the Japanese z in *azaras* to my ear.

The curve of acquisition of the names of the mammals is of the same general character as that for the birds. The highest point was reached in the second experiment three days earlier, namely on the 9th and 10th days, and was slightly higher, the greatest rise in the net number of mammals on any one day being nine, and the greatest number of new names added being eight, both on the 9th day. There were 3 other marked apexes, namely on the 6th, 12th, and 15th, and, for number of birds only, also 18th day. This makes a strikingly regular curve, the apexes being all 3 days apart with a strongly marked dip between. In fact it is precisely similar to my curve for the birds. After an interval of a week he took the same amount of time to name the mammals, namely 12 minutes. He still remembered all except *Edelhirsch*, the last learned, and skunk had degenerated again into hunks. He also mispronounced zebra (deba), *volverenne* (bolbenne), and *dauphin* (dauphinna), and said hockena at first for hedgehog and cacasseux at first for *paresseux*, though in both instances he corrected himself.

In the learning of English words the length of the word had little if any influence upon the time required; for the one-syllabled, two-syllabled, and three and four-syllabled words respectively, required on an average 8.7, 8.6, and 8.7 days to learn. Indeed if one omits the one four-syllabled word rhinoceros, which was especially difficult, the average for the three-syllabled words is 8.3, showing a slight improvement upon the shorter words. It was different, however, with the foreign words. Here the corresponding figures were for French words 12.3, 13.5, and 15 days, and for the German 8.5, 10.9, and 13 days. The longer the foreign word, therefore, the harder it was for this child to master.

From time to time the child would make allusions to the animals in his play. The second day he said to me, "Djou portupine" and called his mama a biesel the 8th day. Three days later he said, "I'm a biber, I'm a muskrat, mama." These occur on the same page. Also, placing his fingers together at the tips, he said, "At's a schnabeltier." On the 16th day, building blocks he said, "cacasseux, at's a gazelle"; the next day, pointing to his boot which he was lacing, he said, "At's a renntier", and of two pencils he had placed in a line, "At's a narwhale, dauphinna, I eat it." Two days later one of his blocks was "a ours"; two days thereafter his boot "a jackal", also his mother (You's a jackal). He used the word opossum the next day and zébu two days later. Some days later he said, "The alleguatehs will eat my schnabeltier all up," and indeed the live alligators threatened to eat the whole dead animal book up in interest. I had taken him to see the alligators at the University. He was frightened by their hissing and kept a considerable distance away; but being alarmed for my safety as I stood quite near them, he came and pushed me away determinedly, saying, "They scare you; those alligatehs are naughty." This was rather brave for a frightened child. When he got safely down stairs he told a professor, "Those alligators are afraid of me!" Thereafter, the alligators were much more real, much more a part of his psychic life, than all the pictured mammals. For example, of his blocks he said, "Look out! You're stepping on my little alleguatehs." And when he sees me getting ready to go out, he says, "Where you goin'? To see the alliguatehs?" or, "to see the 'coons?" (live 'coons at the University) whereas the "raton" is little more than a word. He also shows increased interest in the parrot now that he has seen a pair at the University, and wants me to draw a parrot more than formerly.

On the 11th day of the experiment, while I was drawing cats and mice, he said "Make a biber, a muskrat, a zobel." On the 13th day he drew two loops with a tail at one end and a dot for an eye at the other and called them "Spitzmaus." The tail was made with the same stroke as the loop and was from a half to three quarters of an inch long; the loops were about 2 inches by a half and two inches and a half by one and a half. This is rather early for any definite form to appear in a child's drawings. He had made several drawings before this, the first being an "owl" when he was 2 years 4 months 18 days old. This also was an oblong, about 4 inches long, considerably wider at the head end where he placed three dots as eyes; there was a tail at the other end. It has been said that the child in its drawings is first and chiefly interested in the human form. This was not the case with my boy. He did make a man after making the

owl, but he has never made one since, though he has made many mice and cats. This is no doubt due to the fact that I have constantly drawn for him cats and mice and birds, especially owls and parrots, and scarcely a single human figure. His man, above mentioned, was rather more complicated than his owl, and the eyes instead of being dots were two round scribbles, imitating no doubt the big owl's eyes I made for him. At one end he drew a narrow oblong continuation and called it a "poke." Then he drew a third figure on the same page, big and round, with a tail at one end and three dots for eyes at the other, and called it a "bin." These words are probably original. His mice and cats are all simply loops or oblong figures with a tail at one end and eyes one, two, or three, at the other. Once when he showed me a "little mouse" with an eye but no tail, I said, "where's his tail?" "Right here", he replied, drawing a line from one end of the figure as a tail. As I write this part of the article, the child is two years and a half old.

While being tested from day to day the child made much less effort to recall than I; as a rule his responses were pretty immediate, and when he did think hard, he generally thought silently. Hence he did not give occasion for so many observations of his method. But, from the very immobility of his vocal organs, so far as I could see them, I should judge that he was chiefly working in the auditory realm, striving to hear the sound that had come to him with the visual image. In such a case he sometimes looked at the animal and at other times straight before him. Of course the words were frequently imperfect when first recalled and several times only one part of a compound word was given; as kingfish (kingfisher), queel (quail), ogle (eagle), wobble (yellow wabbler), tart (redstart), tomy or petel (stormy petrel), chem (*chevrotain*), and hilsch (*Edelhirsch*). Of the mammals I counted no incorrect pronunciations as correct unless I felt it was the best the child could do with his present powers of articulation. There was very little mixing of names, as mawkten for martin (confused with magpie on the same page) and lockin for lark confused with mockin his usual word for martin; but there were many cases of substitution with the birds, though very few with the mammals; examples are swallows for doves, lark (and another time "lock, lockin") for redstart; stork, macaw, eagle, grosbeak and parrot for hawk at various times, macaw for manatee, "wild" (boar) and do-bär for See-bär. With the child this was probably oftener a confusion of the animal than of the name, with me it was a confusion of the name. I noticed very few instances of his audibly correcting himself. One was "fish, fush", and later "fush, thush" said two suc-

cessive days, another "lockin, no seh, mama, lark"; in two other cases he got the last part of the word first and it suggested the whole word, namely, "finch, goldfinch" and "bu, zébi." Very often the first syllable or the first letter or even the placing of my vocal organs in position to begin the word was sufficient to suggest it to him; as for example, h- hawk, hum- humming birds, ma and the next day m- magpie, ra- *raton*, r- *renard*, v- *volvenne*, n- narwhale, chev- *chevotain*, e- *Edelhirsch*. Examples of the persistence of a wrong association are "lockin" and "moss" frequently for lark and moose, and the naming of the yellowbird "white-bird" always before saying yellowbird, once he had made this mistake. By analogy with yellowbird and umbrella-bird, he would sometimes say canady bird and heron bird, and tickabirdie (chickadee). He called the secretary bird snake-bird from the snake in its bill before he could name it by its proper name; and he called his birdbook parrotbook from the large and gaudy picture of a green parrot on the cover; while of his mammal book he once said "I want to see the book daddy-lion and mama-lion" from the picture of the two lions' heads on the cover which he had been told were the daddy lion and mama lion.

IV. Mammals Memorized by Adult

With Mr. Kakise's aid I learned the Japanese names for the 52 mammals in the same way I had learned the 48 Japanese bird-names. The birds had required 12 days to be memorized, the mammals required 13 days, or an exactly proportionate time. I did not allow myself to be granted so much time to strive to recall the names as in the previous experiment, using an average of only 19 minutes per day instead of 22, or 4 hours 26 minutes in all. It would, therefore, appear that there is nothing to be gained by an excessive expenditure of time and effort in recalling. I missed only two words on the 12th day and also on the 13th. On the 14th day I said all correctly in 4.5 minutes, and did the same the next day. It is rather odd that in repeating the names of mammals, the child and I took on an average the same amount of time per day, namely 19 minutes. But he required 20 days to learn them to my 13. His maximum time per day was 30 minutes, mine 22; his minimum 12 minutes, mine 4.5. A week later I still remembered all except that I confused the words for sea-lion and manatee (Kai-hyo and Kai-giu). The child after an interval of a week missed only *Edelhirsch*, which, however, he pronounced correctly when prompted. He mispronounced several, however, saying *hunks* (skunk), *portupine*, *dewaff*, *bolbenne*, *deba* (zebra), *dauphinna*, *cacasseux* at first for *paresseux* and then, when I said "no," corrected himself, and in the same way first called

hedgehog *hockena*, no doubt being influenced by the French words ending in a nasal. I required six minutes, and the child twice as long. Then, after an interval of six more weeks, we both tried again. I had forgotten 15 and required an hour for the effort. The child had forgotten 35, but he used only 25 minutes in trying to recall the names, not being desirous or capable of making so much effort as I.

Beginning with the second day, the number of mammal-names recalled by me up to and including each several day, and the number of mammal-names actually recalled that day were as follows:

1, 2, 5, 11, 16, 28, 34, 39, 44, 46, 51, 52, 52
1, 2, 5, 11, 14, 28, 31, 37, 43, 43, 50, 50, 52

In 14 cases I missed words previously known; my child missed 41 times. This does not greatly differ from our record with the birds. My curve of acquisition rises and falls with much the same regularity as before, except that the intervals are one day instead of two, the apexes being on the 5th, 7th, 9th, 10th and 12th days.

There is a remarkable rise on the 7th day of 12 new names and 14 net additional names. That day was a Tuesday and the rise was apparently due to the fact that I omitted Saturday and Sunday repetitions during the course of this experiment, but not of the former one. Through not repeating the words on Saturday and Sunday, my score was little increased on Monday but more than proportionally increased on Tuesday. On the following Tuesday there is another sharp rise of 5 new and 7 net additional names. In the number of names recalled there was a greater rise on these two Tuesdays than on any other day; while on one of the Tuesdays (the second) was only one word missed that had been previously learned, and on the other Tuesday none. On Monday I had to expend much energy in recalling words that the two days' rest had partly obliterated or sunk deeper below consciousness. On Tuesday I got the full benefit of the general Sunday rest in the ability to turn my energy to the acquirement of new words without the embarrassment of having to hold on to half-learned words. The relative loss of Monday is more than made up on Tuesday. It is possible that the long interval had contributed to the fixing of the words in the mind, though not in a way to be readily recalled at first.

A careful examination of these notes shows that my method of recalling these words varied little from that used with the others. Association (j) was more used this time, however, and was of great help. The first and third words recalled were recalled by its means, and altogether this method, without any systematic effort to exploit it, proved possible and helpful in 17

cases, or one-third the number of words. The mind naturally seizes any suggestion of this kind, however remote. In ten cases the association was with English, French or German words, in four cases with Japanese words; in three other cases a connection was seen or imagined between the sound of the word and the appearance of the animal. In only three cases were words so memorized forgotten, and then only partly forgotten. The method of trial and error (f) is the usual one when the word does not at once recur or when I have no association in mind by means of which to recall it. The word is usually built up gradually from day to day, beginning often with a mere feeling that it is *there*, or a curious sense of the character of the word. And just as the word becomes more and more clearly conscious, so it gradually fades out of consciousness, until but a syllable or a letter (not necessarily the first or the last), or an indistinct perception of its vowel sounds or its length, or its rhythm, or even only a vague feeling of familiarity remains. In only three cases was the word completely lost after an interval of six weeks.

Such an experience makes one rather incredulous of Dr. Titchener's somewhat clear cut division of consciousness into two states. If, applying his theory to the memory of a word, he calls the clear consciousness of the whole word one state, and all states of consciousness below this another, he will, of course, count two. But these other states are indefinitely many and diverse, from a vague feeling of familiarity, the ghost of the word that may or may not take on flesh, to the feeling that the word is not quite right, though only one letter or sound is astray.

V. Third List of Animal-names Memorized by Adult with Addition of Visual Word-sign

To see what difference the addition of the visual image of the word would make, a third list of 52 animals was selected, and the animals were shown me day after day, including Sunday, by Mr. Kakise, while he both repeated to me and showed me their Japanese names. Six of the words proved to be ill-chosen, as being too much like words I had already learned, and were rejected and others substituted for them the fourth day (one) and the seventh day (five). Leaving these six out of account, the other 46 names were mastered in 10 days, or in three days less time than the 48 bird-names and in four days less time than the 52 mammal-names. The additional six names, though introduced so late, were all recalled with the rest on the 11th day, except one, and this long word (nokogirizame) was finally recalled with the rest on the 13th day, which was the 7th from its introduction. It may be fairly in-

ferred, therefore, that the addition of the visual image of the word will hasten by three days my memorization of about 50 Japanese names of animals. The total amount of time given to this third series of words, however, was greater than that given to the others, as the average time each day was longer, being 26.6 minutes per day for the ten days and 24.5 per day for the 13 days. The maximum time per day was 33 and the minimum (12th and 13th days) 14 minutes. The six words introduced late got, of course, less time than the others, and, what is of far greater importance, fewer repetitions.

Beginning with the second day (Wednesday), the number of names recalled up to and including each several day, and the number of names actually recalled each day were respectively:

1, 5, 9, 15, 22, 30, 40, 44, 46.
1, 5, 8, 14, 22, 29, 40, 43, 46.

It will be seen that names once recalled were dropped but four times during the experiment, a much better record than when no visual image of the word was allowed. A curve plotted from these figures shows an almost steady rise to the 8th day, in this differing from the previous curves. A certain periodicity is shown as before by the apexes at the 3rd, 6th, and 8th days, but the first two are not succeeded by so marked falls as in the former experiments.

After an interval of six weeks I found I could only recall 14 of the 52 names, though I tried for an hour. Among the 14 was only one of the six names that had been introduced late. This leaves 13 words recalled out of the 46, or 33 forgotten. This is a worse record than the child's, for the list of mammal-names, half English and half foreign. And it is two and a half times as bad as my own record with the list of mammals learned without the visual image of the word. This is probably sufficiently accounted for by the fact that in being memorized the second list of names was repeated on 15 days, while the third was only gone over on 13 days, and still more by the fact that I was tested on the second list after a week's interval, but not on the third list. The child also was tested on his list after a week's interval. But even when due weight is given to these considerations, it would appear that the addition of the visual image, while it makes the foreign word easier to learn, makes little difference to its retention, partly because one makes one's own visual image of the word in time, if not given one and the lack of the visual image makes for a deeper motor and auditory impression. It may also be inferred from this study that a given amount of time accorded to the learning of language will produce better results if distributed over many days than if lavished on a few. In the 16 repetitions of

the second list (the 16th being after a week's interval), I occupied 4 hours 36.5 minutes; in the 13 successive repetitions of the third list I occupied 5 hours 18 minutes with the additional advantage of the visual image; and yet the former list was very much better retained. And the slight expenditure of time (six minutes) devoted to the repetition of the list, with effort to recall, after the interval of a week, which made the 16th repetition of the second list, undoubtedly had much to do with my comparative success in recalling these words after an additional interval of six weeks. All this suggests to the educationist the great value of periodic repetition and reviewing.

I next set about ascertaining how many repetitions I should require in order to relearn the words so badly retained. I accordingly had myself prompted in those I did not know and tried the list again the following day, when I succeeded in half an hour in recalling 44 out of the 52, again recalling only one of the extra six words. The next day I recalled 49 in 11 minutes, two of those missed being still of the aforesaid six. The following day all were recalled in 7 minutes. Three repetitions, therefore, proved necessary for the retaining of the words. But a week later five were again missed, one of them belonging to the extra six.

I put my little boy through the same process with his English and foreign mammal-names. After the six weeks' interval he remembered 17 of the 52 words. Half of the others he could give me when I uttered the first letter or syllable. The 17 words recalled were *Wiesel*, fegwet (ferret), *Sobel*, kangaoo, guinea-pig, oposs, *Dachs*, muskrat. *taureau*, *zébu*, tapir, wild boar, cambels, dewaff, gazelle, goats, seals. He said aw for Wal, loute (*loutre*) for *ours* and gazelle for *Esel*. In relearning the words he, like myself, made very great progress the first day and then rapidly slowed down. He required three times as many repetitions as I to relearn 52 words. Sometimes he said them all at one sitting, at other times he took but a part of them at one period. I let this depend on his own willingness. The numbers recalled at each repetition and the time required were as follows:

		English	German	French	Names
1. One period;	25 minutes:	12	3	2	=17
2. Two periods;	28 "	18	8	6	=32
3. Three "	25 "	23	9	8	=40
4. Two "	20 "	24	12	8	=44
5. Two "	25 "	24	10	9	=43
6. One "	10 "	26	11	9	=46
7. One "	12 "	26	10	10	=46
8. One "	12 "	26	13	10	=49
9. One "	12 "	26	12	12	=50
10. One "	15 "	26	13	13	=52

Those missed at the 8th repetition were *renard*, *urs* and *volverenne*, at the 9th *raton* and *Edelhirsch;* on the last day he hesitated a long time at *Renntier* and *Edelhirsch*, first saying *renard* for the former. Instead of the 21 repetitions, therefore, which had been necessary to learn the 52 words in the first place, he now required 9, while I had required 3 repetitions instead of the original 9 to learn 46 words. It is evident that the French words were still harder than the German, and the German words much harder than the English. All the English words were known on the 6th day.

Some of the more interesting examples of the play of memory are missmaus-krismaus-*spitzmaus*, also spitzmousies, phinphin-*dauphin*, also phana for dauphin, muskox-muskrabbit-*muskrat*, stunk for skunk, ants for *ours*, dit, hed, ketchog, and hedgekok for hedgehog, bolepol for *volverenne*, caresseux and caresseute for *paresseux*, mayma and ibis for manis, fanton for *raton*, and wynocewofewos for rhinoceros. The child gave evidence of the use of methods a, b, c, d, f, i, l, n, q as detailed on pages 331 f.

Some of his remarks during this experiment were: he's cwawlin along, I guess (of the duckbill); he's goin to lie down, I guess (of the porcupine); look at the black feet (of the muskox); they fightin togeddeh, aren't they? (of the reindeer and stag standing quietly facing each other). He likes the monkeys far better than any other animals in the book, and frequently asks to see them. He is doubtless attracted by their grotesque resemblance to human beings. At the page containing anthropoid apes I asked him "What do the monkeys look like?" and he replied at once, "They look Jack, I guess." Being asked which looked like Jack, he pointed to the chimpanzee.

I now tried to get him to learn the English words for the animals whose French and German names he had learned, but it was in vain. I tried for a week and decided it was no use to try longer. The first day it amused him and he pronounced the words after me well and easily; but throughout the week it was impossible to get him to say the English words of his own motion. He would insist on the foreign word and would always say when I prompted him, "No, not dolphin, *dauphin*, not sable, *Zobel*, not otter, *loute*," etc. The only exception was beaver, which he would sometimes say for *Biber*. He even retained the v sound in *Wiesel* and *Wal*, and the French pronunciation of *zébu*. Two weeks later I showed him the book again and we went through it. He was particularly interested in the red squirrel through having seen several (gray) lately in a park. Again he substituted no English word for the foreign except beaver. He had seemingly forgotten six of the

words again, namely, *loutre*, *renard*, hedgehog, *volverenne*, *raton* and *Edelhirsch*, and said caresseux for *paresseux* and veva for zebra. He was then, at the close of the whole experiment (15th June), in his thirty-third month, a healthy, active, naïve boy of possibly more than average alertness, intelligence, and energy of will.

Toward the end of May the boy was taken twice to the Agassiz Museum and once to Barnum and Bailey's menagerie. The first day at the museum he named correctly the following animals: kingfisher, owl, fish, monkey, *renard*, *Dachs*, 'coon, *dauphin*, cats, goats. It is interesting that the real raccoon which he had seen before alive as "coon" and far more frequently pictured as "*raton*", was now called 'coon. (In this connection it might be mentioned here that several days later when we came to the raccoon in the animal book and I called it so, he said "Did you see some the University?" and added "Where are de alligatehs?") The following were named incorrectly, the name he gave being in brackets: another raccoon (opossum, rabbit), caracal and lynx and leopard (tiger), seal (muskrat), roebuck and chamois (narwhale and giraffe), a seal with head raised high (sea-lion). Two birds which were quite unknown to him, the curasoa and turnstone, he called respectively stork and dove. I did not correct him in any instance and the next day he was taken again. This time he named correctly monkeys, bats, deer, narwhale, *dauphin*, owls, ibis, rabbits, giraffe, moose, guinea-pigs, tiger, kingfisher, crow, blackbird, camels. For the following he said the names in brackets: beaver (muskrat), leopard (tiger), gazelle (goat), opossum ('coon), zebra (horsie, tiger), sloth (monkey), flamingo (ibis). Again he was not corrected and the same day after lunch and his usual midday sleep I took him to the menagerie. There he named correctly camels, giraffe, elephant, monkeys, lion, deer, goats, zebra (first called *Esel* which occurs on same page as zebra in his book. The leopard was called tiger and the rhinoceros *Flusspferdt*. The following, of which he knew the pictures, were not named: tiger, kangaroo, tapir, lama, bear, wild boar, zebu. I think he knew the tiger, but would not name it. Three days before, seeing a big gray domestic cat, strikingly barred on legs, head and tail, he had remarked spontaneously first "he 's gray", and then "he 's a tiger." The great number of animals rather bewildered him and he took little interest in naming them, and indeed a less demonstrative interest of any kind than I had expected. But animals in cages are little better than stuffed animals in a museum, of which he had seen a great number the same day and the day before.

My method in my third experiment in the learning of Japan-

ese names of animals differed from that used in the others by my greater use of visualization. The relative infrequency of methods *e*, *f* and *m*, compared with the former experiments, shows that I trusted less than before to the memory of the vocal organs and more to the visual image. This may be one reason for my failure long to retain the memories: the motor memory is probably more tenacious than the others. On the other hand I found myself using and profiting by the visual word image less than I had expected I should. My mind seems to have got into auditory and motor channels for this purpose more than is usual with me. Is it not possible that a man's visual or auditory or motor-mindedness may be a result of habit, environment and training more than of a native condition of the mind? Visualization of the word did occur, but I felt it was a much less prominent factor than it would have been had I used it also in my earlier experiments.

The decreased reliance upon the vocal organs to retain words probably accounts in part for the numerous cases of complete disappearance of a word. There were 16 such cases as compared with but two in the former experiment. A visual image if retained at all would be more likely to be complete than a motor impression, as the former is one act of the eye and taken in by one effort, all parts being equally clear, whereas the motor image is a series, if the word is of more than one syllable or even letter. One sound, on account of emphasis or some peculiarity that made it of especial interest, would be likely to make a deeper impression upon the vocal organs than the others, as, for example, the sound *k* in *kairi*, which alone was retained in the former experiment, *n* in *kamonohas*, *a* in *roba*, *aras* in *yamaras*, *zangko* in *zenzangko*, its repetition then drawing out the rest of the word, etc. Of the words *lost* 87% (13 out of 15) left some conscious trace behind in the auditory-motor experiment, and but 58% (22 out of 38) in the experiment in which the visual image of the word was added. On the other hand the visual image seems to make for clearness, though not for permanence, of impression, for in repeating the series learned without the visual image I six times failed to recognize words after I had said them correctly; while in the series with the visual image of the word this happened only once.

Association was used rather more in the third experiment than in the second, namely, in some 24 words as compared with 17. Eight of the 15 words retained six weeks had been learned at least partly by its aid; in the former experiment this was true of 14 out of the 37 retained. Six of the associated words were completely lost in the last experiment, none in the previous one. Sometimes the association alone was recalled, not bringing with it the desired word.

It might be supposed that the retention of a word would depend upon its length; and, indeed, I found that a word's length not only materially affected the facility with which the word was learned, but also had considerable effect upon ease of retention, when it had not been learned with the aid of the eye. The number of Japanese words of the several lengths in the three experiments and the average number of repetitions required to learn them are as follows:

5	one-syllabled	words	required	3.4	repetitions
45	two-	"	"	4.7	"
32	three-	"	"	6.2	"
40	four-	"	"	6.6	"
24	five-	"	"	6.9	"
5	six-	"	"	6.2	"
1	seven-	" word	"	9	"

The longer the foreign word, therefore, the more difficult it is to memorize. This also was the child's experience.

The 48 words of the first series contained 170 syllables.

The 52 words of the second series contained 154 syllables.

The 46 words of the third series contained 154 syllables.

The 6 extra words of the third series contained 26 syllables.

There were more long words in the third series than in the second; yet the third series was mastered more quickly, another evidence that the addition of the visual image makes for the rapid memorization of foreign nouns. The 14 words of the third list that were retained, however, contained on an average 3.6 syllables apiece, while those lost contained 3.4 syllables, a slight difference in favor of the retention of the longer word. In the case of the second series, however, the difference of 0.7 was in favor of the shorter word, and in the case of the first series it was very marked, being 2.2 in favor of the shorter word.

In the case of the child the difference was 0.3 in favor of the retention of the shorter foreign word, and 0.2 of the native word. It would appear, therefore, if what I have found for my boy and myself proves true generally, that not only are shorter foreign words much more easily learned by both child and adult, but they are also more easily retained. But in the case of the adult, the shorter foreign word was not retained better than the longer, when the words, in being learned, were seen as well as heard.

VI. Tentative Conclusions, Psychological and Pedagogical

1. In learning lists of some fifty names of animals this child of about two and a half years added 2.33 words each repetition to his store of memorized words; I added 4.35 (4 without the

visual word-sign): so that I learned the words nearly twice as fast as the child.

2. Of a list of 52 names of mammals, the child's list being half English and half French and German, and my list Japanese, the child had retained 33% and I 71%, or more than twice as many, after a six weeks' interval.

3. After a six weeks' interval this child required a little less than half as many repetitions to relearn 52 names (one-third of which had been retained) as to learn them in the first place; I required one-third as many repetitions as at first, when I had retained about the same proportion as the child. I relearned them in one-third the number of repetitions that the child required.

4. An adult will learn foreign names faster than a child and remember them better. A child would therefore appear to have no advantage over an adult but rather the contrary in any method of learning a foreign language. It is probably chiefly the adult's trained and developed capacity of attention that gives him the advantage. His wider linguistic experience is also of importance, giving him many helpful associations to aid his memory. Meumann and his assistants found a similar advantage on the part of adults when compared with older children.

5. At two and one-half years the child's linguistic habits are already sufficiently formed by the acquirement of his mother tongue to make the acquirement of a foreign language considerably more difficult than that of his own. To an English-speaking child French is more difficult of acquirement than German.

6. The longer the *foreign* word, the harder it is both to learn and to retain, for both child and adult. With the visual image, however, I found long words at least as easy to retain (though not to learn) as short ones. The length of *native* words had little perceptible effect upon facility of acquisition in the case of the child, and but a slight effect upon ease of retention.

7. Words interesting as sounds whether from agreeableness or some other striking peculiarity are easy to retain, as *chickadee* and *macaw* for the child and *nemurinezumi* and *tokaké* for myself.

8. Words hard to pronounce are hard to learn and retain, as doves, secretary-bird, stormy petrel, musk-ox, hedgehog, zebra, *renard, volverenne, raton, Edelhirsch, Flusspferdt Renntier* for the child, and *nokogirizame, hikuidori* and *saezoridori* for me.

9. With the child the auditory image of a word seemed to be very closely associated with the visual image of the animal, the latter generally calling up the former directly and promptly. With me the motor impression seemed to be more important,

the ear serving as a more or less necessary critic of the efforts of the vocal organs to form the word. The visual image of the word was generally of least importance.

10. The addition of the visual word-sign made fewer repetitions necessary for my learning of words, but was of doubtful advantage for their retention. The lack of the visual image seems to make for a deeper motor and auditory impression and a solider attainment. The visual word-image makes for completeness rather than permanence of impression; the motor impression is more lasting though not so clearly and completely conscious. I ought to add that I have good visual as well as auditory imagination.

11. A foreign language may be learned more rapidly with the aid of the eye, but will be a more permanent possession if the attention is mainly focussed on the ear with consequent increased and *correct* use of the vocal organs. If a modern language teacher will read a great deal to his pupils in the foreign tongue and will use it himself a great deal in class, articulating with great care and accuracy, the pupil's ear will be trained to correctness of pronunciation and accent and his vocal organs will also unconsciously be exercised in the utterance of the correct speech which alone the pupil has heard and for which his ear is attuned. Thus the vocal and auditory organs will be constantly and more or less mechanically trained together, without the pupil's ever uttering a word; for we naturally repeat with unconscious movements of the vocal organs that which we hear. After some months of such unconscious learning, the pupil may be allowed to speak and read the foreign language and he will be found able from the start to do this with unwonted correctness and facility as regards pronunciation.

12. A given amount of time devoted to memorization will be more permanently effective if expended in frequent repetitions with considerable intervals, than if concentrated in fewer study periods, as discovered by Ebbinghaus with meaningless syllables and later demonstrated by Jost.

13. The actions of an animal attract a child's interest most, its form next and its color least. For this reason the child greatly prefers seeing the animal itself, especially when free, to seeing its picture, however good. Pictures of animals for children should represent them as doing things, fighting, running, chasing, climbing, swimming, drinking, etc. It is evidently not action, form and color as such, *i. e.*, as abstractions, that this child was interested in, but it is these as expressions of meaning, as interpretations of animal life. The action of the animals was fullest of meaning to this child, and consequently of interest, the form came next, and color had the least meaning.

Mere exercises in sense-training divorced from living interests are unpsychological.

14. The power of observation and redintegration, the ability intuitively to associate the various parts of which an object is composed with the object as a whole, is remarkably developed in the little child of two and a half years. There would seem to be no hard and fast line between intution and judgment.

15. Formal, direct teaching with little children is confusing and futile.

16. The curves of acquisition of various lists of some fifty names per list all showed from three to five marked periodical rises and falls, the recovery being generally slower in the case of the child than in that of the adult.

17. Work done under conditions of fatigue not only is harmful physically, but results in mental loss and deterioration. Time spent in study by a fatigued brain is worse than wasted.

18. A child of two and a half years has little power of continuous attention and he may suffer for a considerable time without giving any perceptible sign of fatigue other than that discovered indirectly by a careful examination of his work. A little child, like a "willing horse", can easily be overworked by appeals to his affection or interests. This is probably true of children all the way up. Our school boards and exploiters of child labor are oblivious of this as of much else that vitally concerns the child.

19. The child's earliest drawings showing perception of form are determined by his interest and experience. In this child's case they were of animals, the first at the age of 2 years 4 months 18 days. If the child's drawings are of any value as a test, reproduction or the recall of free memory images must be much later and more slowly developed in him than recognition. This would indicate that his wonderfully keen perception of form and action involves but a vaguely conscious analysis, is in fact very intuitive. Children would seem to know more than we give them credit for, but are deficient in power to reproduce and to express it. My child of three does not recognize the cruder drawings of men, horses, clocks, etc., made by children of his own age, and very similar to those he makes himself.

20. The phenomena of memory throw light upon the question of degrees of consciousness, showing them to be indefinitely many. A word may be built up in the mind by gradual stages of clearness and completeness and as gradually disappear.

SEX DIFFERENCES IN THE TAPPING TEST: AN INTERPRETATION

By FREDERIC LYMAN WELLS, Ph. D., Assistant in Pathological Psychology in the McLean Hospital, Waverley, Mass.

While it is doubtful if we ought to attach to the speed of repeated voluntary movements the fundamental significance accorded to it by some of its earlier investigators, certain sex differences have appeared in the writer's experience with the test that would seem to justify a brief presentation. The subjects whose results form the basis of the present discussion are ten men and ten women, with two exceptions nurses in the McLean Hospital, and of corresponding age and environmental level. It would be very difficult under other conditions than the present to equal these groups of subjects in homogeneity, co-operativeness, and unaccustomedness to the conditions of psychological experiment. For this reason, a significance may perhaps be attached to the present results which the limited number of subjects might otherwise largely vitiate.

The experiments with the ten men have already been described in detail,[1] and those with the ten women are essentially similar. Two experiments are performed with each subject, each consisting of five 30″ series of tapping with each hand, the right hand preceding in the first experiment and the left hand preceding in the second experiment. From the results of these experiments as performed, we derive various functions, the principal ones with which we are here concerned being as follows:

1. The relation to each other, in respect to gross rate, of five successive 30″ series of tapping with the same hand, the series being separated by rest intervals of 2′ 30″.

2. The gross fatigue effect, as given in the "index of fatigue," (f) which index is derived by dividing the average number of taps in the last five 5″ intervals (25″ of tapping) by the number in the first 5″ interval.

3. The fatigue curve, or curve of the decrease in the number of taps executed during the six successive 5″ intervals of a 30″ series.

4. The relation of the right hand to the left hand, as given in the "index of right-handedness" which is the average num-

[1] *American Journal of Psychology*, XIX, 1908, pp. 437 ff.

ber of taps executed in five successive 30″ series by the left hand, divided by the corresponding average for the right. The higher this index, the better the left hand in proportion to the right; above 1.00 the left is absolutely superior to the right.

5. The mean variation of successive experiments in the same individual, *i. e.*, the constancy of his results to each other.

We shall first examine those aspects of the results concerned with differences in the average of performance, and later those functions which deal with the relative variability of the sexes about these averages.

The previous researches which afford material for the study of sex differences in the tapping rate are concerned mainly with children and adolescents. While they do not maintain absolute correspondence, their general indication is that the boys are faster than the girls, and that the sex difference increases with age, so that we should expect to find here a consistent superiority of the men over the women.

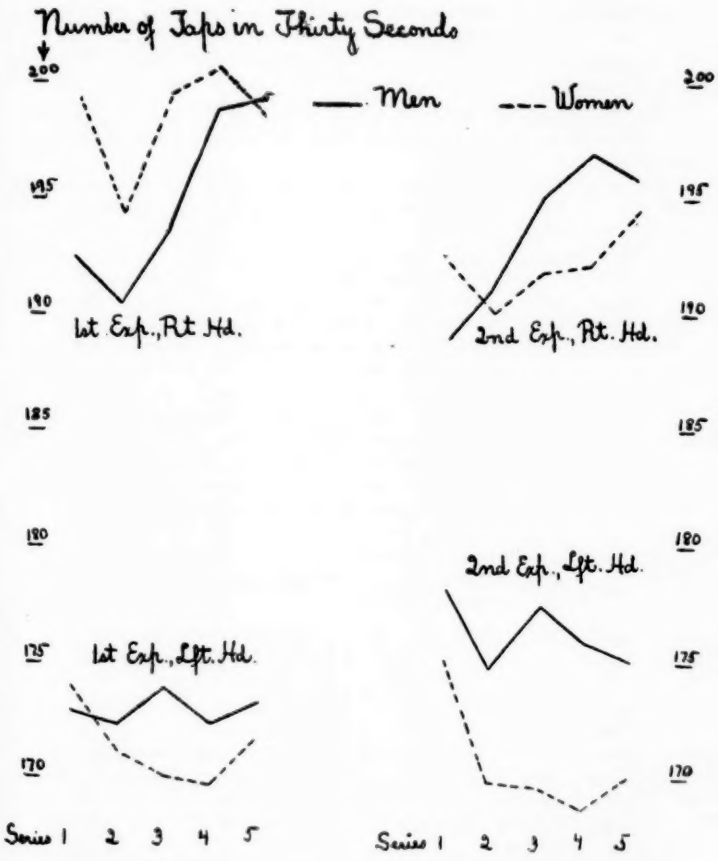
The actual results are best presented in the form of curves, which give the total number of taps executed in each of five successive 30″ series of tapping. The continuous lines represent the tapping rates of the men, the dotted lines those of the women; the right and left hands, and first and second experiments being as indicated on the curves themselves. Thus in the first series of the first experiment with the right hand, the men execute 192.4 taps, the women 199.4; in the fifth (and last) series of the second experiment with the left hand, the men have 175.1 taps, the women 170. The curves give the fluctuations in rate from series to series. (Plate I.)

Briefly, the women surpass the men in the performance of the right hand in the first experiment. Elsewhere they are inferior, and they are much more inferior in the second experiment than in the first. The women are more favored in the first experiment, by virtue of its being the first, than the men, and they lose more in the second experiment, as such, than the men. The first experiment represented the first contact of any of the subjects with the test. There is thus of course a primary *Neuigkeitsantrieb* which is absent in the second experiment. This scarcely affects the men at all; indeed, their second experiment is as a whole somewhat better than the first. On the other hand, the women are in the second experiment markedly inferior to their first performance, in the absence of this *Neuigkeitsantrieb*.

There is a corresponding sex difference in the fluctuation in rate of the successive series, as illustrated in the curves. Just as the first experiment has a special *Neuigkeitsantrieb*, so is there a secondary *Anfangsantrieb* in the first series of each record of either hand. If the curves are examined, it will be

seen that this first series is, relatively to the rest of the curve, very much higher in the women than in the men; *i.e.*, the women also indicate more strongly the presence of this secon-

PLATE I



dary *Anfangsantrieb*. The indication of *Schlussantrieb* at the conclusion of each experiment (the last series with the left hand in the first experiment, and that with the right hand in the second) is also more marked in the women.

As regards susceptibility to fatigue, previous studies, while differing somewhat from the present in manner of calculation, leave a slight balance of probability in favor of the men as the less susceptible to fatigue. As given in terms of the *f*, the present experiments average as follows :

Comparative *f* 's of the two groups.

	R1	R2	L1	L2
Men	.90	.91	.88	.90
Women	.90	.91	.87	.89

(In the tables, R1, R2, L1, L2 will be employed to indicate the first and second experiments with the right and left hands respectively.)

That is, the last 25″ tapping averages from 87% to 91% as fast as the first 5″. There is no immediate sex difference worth mentioning, especially when we find that the mean variations average about .03. Such infinitesimal differences as exist are in favor of greater fatigue immunity for the men. It is noticeable that in both men and women the left hand is more susceptible to fatigue than the right, and the first experiment more so than the second, this latter presumably a manifestation of less *Antrieb* in the second experiment.

The writer has previously mentioned that in the tapping test the fatigue phenomena seemed remarkably independent of the fatigue sensation accompanying the work. This generalization was based on experiments with men only, and the results with the women do not bear it out so well.[1] Each of the women was asked whether sensations of fatigue were present, and if so, in which hand they were the more prominent; it being then noted whether greater fatigue sensations on one side corresponded with greater fatigue phenomena on that side. The results were as follows.

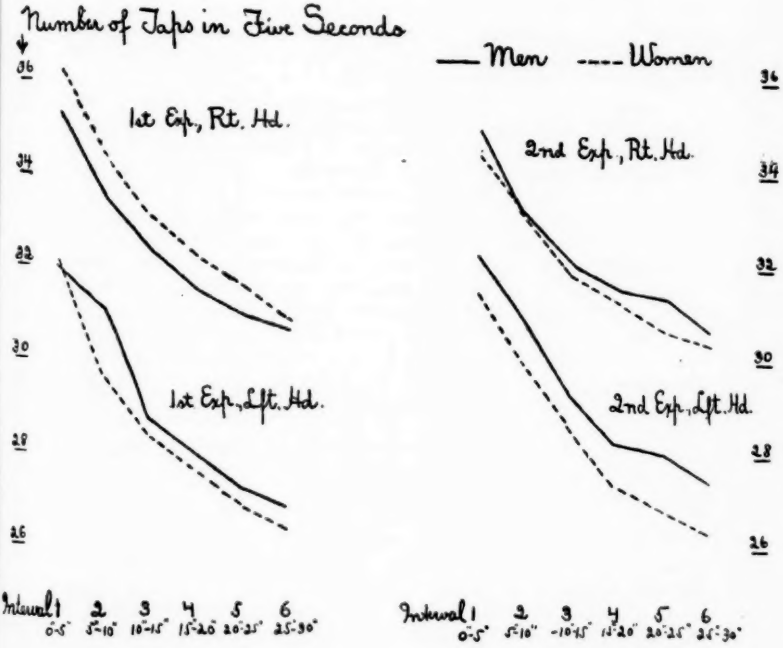
Sensations of Fatigue	*Phenomena of Fatigue*
None	No significant difference
Equal	Equal
Only in left	More in left
More in left	More in left
More in left	No significant difference
More in left	No significant difference
More in left	More in left
More "tension" in right	More in left
More in right	Equal
Probably more in right	More in left

We obtain a positive relation in five cases, four are equivo-

[1]The experiments with the men antedate those with the women by about a year.

cal, and one is negative. This correspondence, while by no means good, is rather better than seems to obtain among the men, though the inquiries with them were not so systematic. If there is a sex difference here, *i. e.*, in that the women's introspective account of fatigue sensation shows a better correspondence with the actual fatigue phenomena than that of the men, its most reasonable interpretation would seem to be that the women's performance is more influenced by fatigue sensation. It should be mentioned that subsequent features in the results on fatigability bear out this interpretation.

PLATE II



The accompanying cut shows the fatigue curves of the right and left hands in each experiment, the curves of the two groups of subjects being superposed. As previously, the continuous lines are the records of the men, the dotted lines those of the women. The curves are of the ordinary shape and

show no characteristic sex difference in form. The relative inferiority of the women in the second experiment, already mentioned, is again evident. Another sex difference may be mentioned here, namely that the women are more inferior to the men with the left hand than with the right, *i. e.*, the right and left hands are farther apart in the women than the men. In the gross, this difference is best expressed in the index of righthandedness, which averages as follows in the two experiments.

	1st Experiment	2nd Experiment
Men	.89	.91
Women	.87	.89

In both groups the index is higher in the second experiment than in the first; *i. e.*, the preceding hand is more favored as such. The writer has previously mentioned this as a normal property of the test; but it is more probably related to *Anfangsantrieb* than to any generalized fatigue effect upon the following hand. It is seen also that the index is throughout lower in the women, the difference being just at the limits of probable error. This shows again that for this function there is more bilateral asymmetry, the right and left hands are farther apart, in the women than in the men.

Up to the present we have been dealing with averages only, reserving the mean variations for separate discussion, since these have a special bearing upon the question of sex differences. It seems to be very generally accepted that at least in most aspects, individual differences tend to be greater in men than in women, and it may be worth while to examine the data from this point of view. Only the mean variations need be presented, since it would be hardly possible to consider the more minute features of the problem of variability from a basis of ten cases of each group.

In initial rate, *i. e.*, rate for the first 5″, as well as in gross rate, *i. e.*, rate for the whole series of 30″, the mean variations of the different groups are as follows.

	R1		R2		L1		L2	
	5″	30″	5″	30″	5″	30″	5″	30″
Men	3.3	14.0	3.9	14.3	3.0	17.6	3.5	15.9
Women	2.8	14.3	2.1	13.8	2.3	13.2	2.8	14.6

The women are practically throughout less variable than the men. In the m. v. of the gross rate (30″) this difference is not so well marked, and for a reason that is worth analysis. Attention has already been called to the fact that there was in the men a marked tendency for fatigue to *decrease* the individual differences. This tendency is also present in the women, but very much less so than in the men; indeed, at the end of the 30″ the variability of the two sexes is practically equal. Under fatigue,

the individual differences of the women become relatively greater than those of the men.

As it previously appeared that there was no characteristic sex difference in the average of the *f*'s, so here we fail to find it in their variability. The mean variations of the *f*'s are practically the same in men and women throughout. As may however be inferred from what has just preceded, this equality is purely superficial; the *f* itself shows no sex difference, but both of its factors (the first and the remaining five intervals) do. Inasmuch as the women are to a certain extent less variable in the initial interval, if they continued to be equally so throughout the rest of the fatigue curve, the m. v. of the *f*'s would show equally the smaller variability of the women; but, inasmuch as we find the variability of the *f*'s equal, or nearly so, it follows that the fatigue curves of the women have under fatigue gradually increased in variability relatively to those of the men.

Absolutely, the fatigue curves of the women (whose average is given in the cuts on p. 357) resemble each other in form somewhat the more closely, as is indicated in the fact that in the women the mean variations of the successive 5″ intervals are consistently smaller than those of the men. It is in this, an important aspect of the results, that smaller variability of the women is the most unequivocally illustrated. The average m. v.'s of the six successive intervals in the two experiments for the men and women are as follows:

	R1	R2	L1	L2
Men	2.4	3.0	2.4	2.7
Women	2.1	1.9	2.3	2.4

In view of what has been said, however, it is more than probable that at the end of 60″ of tapping the variability of the women would exceed that of the men.

It was noted previously that the right and left hands are farther apart in the women than in men. It is also observable that the difference between the right and left hands is more variable in the women than in the men, the figures being as follows.

	1st Experiment	2nd Experiment
Men	.035	.033
Women	.047	.041

The relationship of the two hands in these experiments is affected mainly by the matter of precedence of either hand in the test. We saw that this factor affected the tapping rates of the women somewhat more than those of the men, and we see here that it also affects them more variably than those of the men.

As an aspect of the same general result, it may be noted that the day to day variability in gross rate was in the women strikingly greater than in the men. Taking the mean variation of the two experiments performed for each hand in each subject, we obtain for the two experiments the following results in the men and the women.

	Right	Left
Men	1.6	3.5
Women	3.4	3.9

That is, the first and second experiments differed much more from each other in the women than in the men. (*Cf.* p. 354.)

Considered individually, one could attach no special significance to these data as isolated facts; and it is much more reasonable to regard them as secondary to other characters of sex difference having a more fundamental value. The results are best discussed in their relation to two such characters, which may be cited according to Havelock Ellis, as the lesser variability of women, and the greater affectability of women. The latter is perhaps the most fundamental sex difference that exists on the psychical side, and indeed, were it not so apparent to everyday observation in general, it could be asserted positively for the group of subjects immediately concerned. This being the case, we must expect that the affective factor in every feature of the experimental conditions will influence the performance of the women to a significantly greater extent than it will those of the men. And in review of the points of sex difference previously mentioned, it seems that, as the sex differences appear most markedly in those features of the experiments in which the affective element is the most prominent, so are these differences most reasonably interpreted as the expression of this fundamental sex difference in affectability. It is, of course, possible to construct other hypotheses which will cover the various points of difference observed; but it is a precarious logical system which interprets with a variety of assumptions a series of observations referable to a known fact.

To this we must add, that besides being more affectable, the women were much more variable in their affectability than the men. This was clearly observable in the differences of experimental attitude. The women went at the test in much less of a routine manner; they more frequently evinced desire to examine and understand the apparatus, and were more likely to express concern as to the object and outcome of the tests, and regarding future experiments. The men showed a more uniform behavior in these respects than did the women, and their attitude toward the test was in general a more "objective" one. Among the women a varying amount of persuasion was occa-

sionally demanded; but it was always apparent that the subjects were really co-operative, and that the seeming inadequacy was wholly the product of an exaggerated affective reaction to the unaccustomed conditions of the experiment.

There are operative in these experiments four special affective influences: the precedence of the hands in the experiment, the sensations of fatigue, the use of the preferred or unpreferred hand, and the relative novelty of the test. The sex differences are marked principally according to the amount which these factors influence the special aspect of the results under consideration.

To particularize, the women are much more susceptible to the influences of *Antrieb* than are the men. The *Anfangsantrieb* of the inception of the experiment was illustrated in the record of the preceding right hand in the first experiment and perhaps even more strikingly so in the second experiment with the preceding left hand. As between the successive 30″ series, the influence of a secondary *Anfangsantrieb* is apparent in that the first series of each hand is in the women relatively superior to the first series in the men. . The apparent failure of the women to bear up so well under fatigue sensations indicates a heightened responsiveness to an affect of the opposite character.

If the greater difference between the right and left hands in the women is significant, an analogous interpretation is possible in that we perhaps try harder in doing what we expect to do better. The subjects naturally expect to do better with the right hand, and so try harder with it; the women, on an average, harder than the men. In a more affectable group, the hands might thus appear, in general, farther apart.

There are certain features of the test which we should hardly expect to be influenced by these differences in affectability, and which in fact do not seem to be so. The individual differences in tapping rate are slightly greater in the men than in the women, and the form of the fatigue curve is also more constant in the men, though it tends to become less so. Here we seem to have a sex difference referable rather to the generally lesser variability of women, already mentioned as a fundamental sex difference. In the remaining results, however, we find this general tendency toward lesser variability in women in conflict with the special factor of the greater affective variability of the women, and we shall find that in these cases, this latter factor is sufficiently potent to more than offset the other, so that the final figures show from almost every viewpoint, a lesser variability of the men.

Mention has been made of the fact that under fatigue the individual differences of the women do not show the same

tendency to decrease as those of the men. As we previously judged the women to be more responsive to sensations of fatigue than the men, we now find them to vary more in their responsiveness to them than the men, the former being an aspect of the women's greater affectability, the latter of their greater affective variability. And as we found the right and left hands to be father apart in women than in men, so now we find the index of righthandedness to be more variable in women than in men; the greater difference a product of greater affectability, the greater variability of the difference a product of the greater affective variability.

The greatest affective difference that is objectively given in these tests is perhaps that between the first and second experiments; certainly the sex difference is very well marked here, the women being relatively much better in the first experiment than in the second. The women respond readily to the *Neuigkeitsantrieb* of the first experiment, and miss more strongly the interest of novelty in the second; a more even experimental attitude is indicated in the men.

To briefly recapitulate, no immediate sex difference was indicated in the absolute rate of tapping, in the gross amount of fatigue loss during the 30″ period, or in the form of the curve of this fatigue loss. More detailed examination of the results, however, indicated sex difference as follows:

1. The women relatively surpass the men in those periods of work most likely to be subject to special *Antriebe*.

2. The introspective accounts of the fatigue sensations given by the women agree better with the objective fatigue phenomena than those of the men seem to do; hence the women are probably more influenced by fatigue sensation than the men.

3. The right and left hands are farther apart in women than in men.

4. In initial rate (5″) the women are considerably less variable, in the gross rate (30″) slightly less variable than the men.

5. In the 30″ work period employed, the shape of the work curve is considerably less variable in the women than in the men, but the women show a tendency to relatively increasing variability under fatigue. (*Cf.* 2.)

6. The relationship of the right and left hands is more variable in the women than in the men.

7. The first and second experiments differ more from each other in the women than in the men.

The above sex differences are found mainly in those features of the experiment which especially involve the affective factor in the subject's attitude; and they are manifestations of the greater responsiveness of the women to this affective element.

When this factor is not especially involved, the individual differences of the women tend to be less than those of the men; but when it is involved they are greater, illustrating not only the presence of greater affect among the women, but also the greater variability of this affect. Thus the differences found are not fundamental sex differences, but are secondary to certain differences in temperament; and in a group of subjects in which these temperamental differences were by any chance reversed, we should expect to see the experimental differences also reversed.

It is very possible that a practiced experimenter can interpret these temperamental differences in the subjective attitude from observations of the general behavior under experimental conditions more reliably than through the crude introspection of a naïve subject. And though introspective accounts of the subjective attitude often seems to add little enough to the significance of the experiment, we might still be exposed to grave error in concluding from this that the subjective attitude does not still influence the results of such observations to a marked degree. In the foregoing, it has been endeavored to indicate the extent to which certain rather elementary motor functions might be influenced by what is perhaps the most important single factor in the subjective attitude, its susceptibility to affective influences.

THE RELATION OF ACCURACY IN SENSORY DISCRIMINATION TO GENERAL INTELLIGENCE

By Professor Edward L. Thorndike, Dr. Wilfrid Lay and Mr. P. R. Dean.[1]

The purpose of this paper is to present certain new data concerning the relationship mentioned in the title and to show the bearing of these data upon the conclusions set forth by Spearman in his *General Intelligence Objectively Determined and Measured* which appeared in this *Journal* (Vol. XV, No. 2) in April, 1904.

The Original Measures

The measurements from which conclusions will be drawn were made by Dr. Lay upon 37 young women students in a normal school and by Mr. Dean upon 25 high school boys (all in the 3rd year of the high school course). The 37 young women drew each 90 lines, 30 as nearly as possible equal to a 100 mm. standard, 30 as nearly as possible equal to a 75 mm. standard and 30 as nearly as possible equal to a 50 mm. standard. They also each filled 16 boxes with shot, 8 as nearly as possible equal to a 100 g. standard and 8 as nearly as possible equal to a 200 g. standard. Each one rated all the rest in order of merit for general intellect, using each her own conception of what general intellect was as the basis of grading. Eight of the professors in the normal school also graded each of them (with a few exceptions in the case of three of the teachers) in the same way. Their scholastic records in the normal school were also used as measures. I use the average deviation from the standard as the measure of inaccuracy in the case of the tests with lines and weights. Some reasons might be adduced for choosing the variability around the individual's constant error instead, but there are far weightier reasons against doing so.

The 25 boys drew lines similarly except that some drew fewer than the 90 while others drew more; made up weights

[1] The shares of the authors in this research were as follows: The original measurements were taken by Dr. Lay and Mr. Dean, who also calculated some of the deviation measures and correlations. The remaining calculations were made by Professor Thorndike who was also responsible for the research and for the account of it here given.

similarly; were estimated similarly by 6 of their own members and 4 of their teachers.

The lines were drawn and the weights made up under the same conditions for all students within each group. The tests covered several days for each individual, so that spurious correlation from fatigue, temporary illness, etc., was reduced to a small amount. Within each group differences of age and maturity are for our purpose so slight as to be negligible. The median deviation of the young women in age was only 10 months and that of the boys only about one year.

We have then for the women fairly accurate measures of accuracy of discrimination of these lengths, accuracy of discrimination of these weights, intellect as judged by one's fellow students and intellect as judged by one's teachers. Such measures from two random halves of the scores correlate to .665, .504, .915, .72 and .62 respectively, which means that the measures used from the entire scores for each individual would correlate with other similar sets to about .8, .7, .9½, .9 and .8 respectively.

In the case of the boys the opinions of intellect of fellow pupils and teachers were combined. We have then for the boys fairly accurate measures of discrimination of these lengths, discrimination of these weights, intellect as judged by one's teachers and fellow students, and scholarship. Such measures from two random halves of the scores correlate to .691, .722, .869 and .873 respectively, which means that the measures used would correlate with other similar sets to about .8, .8, .9½ and .9½ respectively. The "raw" correlations from which we have to argue are consequently subject to only very moderate "attenuation" from chance variations in the obtained measures from the true measures for which they stand. The number of cases is sufficient to determine close correlations with a very small margin of probable deviation from the true result. When the relationship is only slight, the reliability of the result is, of course, much less, but is still sufficient to prevent insecurity in any of the general conclusions which are of interest.

To these conclusions I proceed at once, referring the specialist in mental relationships to the detailed table at the close of this paper.

The Measures as Samples of 'Intellect' and of 'Sensory Discrimination'

Intellect as judged by teachers and intellect as judged by fellow students are much the same thing. The raw correlation in the case of the woman students is .85. This becomes about .95 when allowance is made for the inadequacy of the original

measures. The raw correlation in the case of the high school boys is .76. This becomes nearly .9 when the Spearman correction is applied.' The women preparing to become teachers naturally weight aspects of intellect more in the fashion of the teachers than do the high school boys. But the congruence of school-boy's and school-teacher's opinion is remarkable.

In the case of the normal school women scholarship is an almost perfect symptom of intellect as they and their teachers judge the latter. The correlation between the combined judgment of fellow pupils and teachers and the scholarship record is .85, which becomes about .95 when allowance is made for the inaccuracies of the latter. And much the same would of necessity hold of the judgment of fellow-pupils alone.

With the high school boys scholarship is by no means a perfect symptom of intellect either as judged by the boys or as judged by their teachers. The correlation is naturally somewhat higher in the latter case, but it is by no means high. These relations are approximately

.6 for scholarship and intellect by the combined judgment,
.6 " " " " " " teachers' judgment, and
.4 " " " " " " pupils' judgment.

This difference is, of course, what should be found from accurate measures, since the students in the professional school do devote their intellects to scholarship, and do, so to speak, measure their intellects by it, more than is the case with the high school boys. Scholarship is, with the latter, in large measure a product of interest rather than ability.

From these facts it is evident that in the case of the high school boys the three measures,—teachers' opinions, fellow-students' opinions, and school marks,—are something like a fair sampling of measures of general intellect. In the case of the women students the sampling is much weighted in favor of the scholarly sort of intellect.

The discrimination of lengths and the discrimination of weights are known to be random samples of sensory discriminations for the very good reason that they were picked at random.

The Relation of 'Intellect' to 'Sensory Discrimination'

From his measurements Spearman calculates that the factor common to school marks and ratings as to 'common sense' by fellow-students and teachers, correlates perfectly with, and hence is identical with, the factor common to discrimination of pitch, discrimination of light intensities and discrimination of weights, and concludes that there exists "a correspondence between what may provisionally be called 'General Discrimi-

nation' and 'General Intelligence' which works out with great approximation to *one* or *absoluteness.*''

The measurements obtained in the present investigation do not in the least support this hypothesis. The correlation between whatever is common to (A) drawing 50, 75 and 100 mm. lengths accurately and (B) making weights equal to 100 g. and 200 g. standards and whatever is common to (C) intellect as judged by fellow-students and (D) intellect as judged by teachers does not come out as 1.00 but as .26 or .15 according as we apply the first or second of Spearman's correction-formulæ. When, in the case of the high school pupils, the two measures of general intellect taken are (C) combined student's and teacher's estimates and (D) school marks, the correlation comes out .29 and .22 by the two methods.

That is, the most probable relation between the factor common to all sensory discriminations and the factor common to intellect judged in these three ways is, from our data, not 1.00, but .23.

It is perhaps best to wait for further and fuller measurements of the relation in question before attempting to explain the difference between this result and Spearman's. But one fact may be noted now. With young children a test designed to measure sensory discrimination may easily become, to a considerable degree, a measure of ability to understand instructions, that is, of one feature of general intellect.

The variety of measures taken and the elaborate corrections made by Spearman make a detailed comparison step by step of his and the present research difficult and in the end unproductive. The essential differences are (1) that Spearman does not give measures of the reliability of his measures of any species of sense discrimination or of any but a few of his measures of intellect and (2) that his material is complicated by age and sex.

The theoretical importance of Spearman's conclusion lies in the support which it would give, if verified, to the hypothesis that the efficiency of what may be called the general mammalian foundation of the central nervous system is closely correlated with what may be called the specifically human neurone-connections. The present results support the contrary hypothesis, that the efficiency of a man's equipment for the specifically human task of managing ideas is only loosely correlated with the efficiency of the simpler sensori-motor apparatus which he possesses in common with other species.

Spearman's other main conclusion is "*that all branches of intellectual activity have in common one fundamental function* (*or group of functions*), *whereas the remaining or specific elements of the activity seem in every case to be wholly different from that*

in all the others." This is, of course, contradicted by the correlation of .23 instead of 1.00, and also by the fact that we obtain a much higher correlation between discrimination of lengths and discrimination of weights than between either one of them and general intelligence. From our figures the correlations, if perfect original measures were at hand, would be about .15, .25 and .50 respectively for accuracy in drawing lines with general intelligence, accuracy in making up weights with general intelligence and accuracy in drawing lines with accuracy in making up weights.

I may add that other studies of correlation made by my students and myself are unanimous in contradicting Spearman's ingenious hypothesis of one sole common element as the cause of all positive correlations. We find, for example, that efficiency in marking A's on a sheet of printed capitals, efficiency in finding circles or hexagons or isosceles triangles on a sheet of printed geometrical forms and efficiency in finding misspelled words are in adults all very closely intercorrelated (to .8 or more), but are by no means so closely correlated to general intellect. In general there is evidence of a complex set of bonds between the psychological equivalents of both what we call the formal side of thought and what we call its content, so that one is almost tempted to replace Spearman's statement by the equally extravagant one that there is *nothing whatever* common to all mental functions, or to any half of them.

I regret that it is out of question to print the original measures. But as the record of each individual of the 62 measured comprises 90 numbers representing errors in drawing lines, 16 numbers representing errors in estimating weights, 10 or 42 rankings in estimating weights, 10 or 42 rankings in intellect and from 12 to 30 marks for scholarship, the total table would require nearly 10,000 entries. The table of correlations follows. Column I gives the results from the women students; Column II gives the results from the high school boys.

TABLE I

	ACTUALLY OBTAINED ("raw") CORRELATIONS: PEARSON COEFFICIENTS	I	II
1.	Scores from half of the 100, 75 and 50 mm. lines with scores from the other half	.665	.69
2.	" " " " 100 and 200 gram weights " " " "	.505	.72
3.	" " " " pupils' impressions of intellect " " " "	.915	
4.	" " " " teachers' impressions of intellect " " " "	.72	
5.	" " " " academic records " " " "		.87
6.	" " " " combined pupils' and teachers' impressions of intellect with scores from the other half " " " "		.87

TABLE I—*Continued*

		I	II
7.	Scores from all the lines with scores from all the weights	.52	.25
8.	" " " " " " " " pupils' impressions of intellect	.25	
9.	" " " " " " " " teachers' " " "	.12	
10.	" " " " " " " " pupils' and teachers' impressions of intellect combined		.055
11.	" " " " " " " " academic records		−.01
12.	Scores from all the weights, with scores from all the lines	.52	.25
13.	" " " " " " " " pupils' impressions of intellect	.235	
14.	" " " " " " " " teachers' " " "	.08	
15.	" " " " " " " " pupils' and teachers' impressions of intellect combined		.205
16.	" " " " " " " " academic records		.21
17.	Scores from all the pupils' impressions of intellect with scores from all the teachers' impressions of intellect	.85	
18.	Scores from all the pupils' and teachers' impressions of intellect with scores from entire academic records		.54
19.	Scores from all the lines and weights with scores from all the teachers' and pupils' impressions of intellect combined	.165	
20.	Scores from all the lines and weights with scores from combination of teachers' impressions, pupils' impressions and academic records		.145

COEFFICIENTS CORRECTED FOR CHANCE VARIATIONS IN THE ORIGINAL MEASURES BY THE SPEARMAN METHODS OF CORRECTION

I.	(A) The factor common to accuracy in lines and accuracy in weights with (B) the factor common to pupils' impressions of intellect and teachers' impressions of intellect	.20	
	(A) As above, with the factor common to the combination of teachers' and pupils' impressions and academic scholarship.		.255

The most probable correlation between "general discrimination" and "general intelligence" is thus .23.

AN APPARATUS FOR THE STUDY OF KINÆSTHETIC SPACE PERCEPTION.

By JAMES H. LEUBA, Bryn Mawr College, Pa.

The accompanying drawing reproduces the apparatus as set up for forearm movements.

The subject sits or stands with the arm over the lever (*L.*). The hand-holder (*H. H.*) is put at such a distance from the shaft (*S.*) that the centre of the elbow joint is opposite the end of the shaft. The forearm does not rest on the lever, but is entirely supported at one end by the hand-holder, and at the other by the upper arm. Thus, only the hand of the subject is in contact with the instrument, unless, instead of making use of the hand-holder, the forearm be tied to the lever by a soft bandage. (See below, No. 5.) When the point of the lever indicates zero on the scale, the upper arm hangs vertically from the shoulder and the forearm is horizontal.

The two essential dispositions of the apparatus are the following:

1. The arm movements produce the rotation of a shaft placed at right angles to the plane described by the moving arm and directed towards the centre of rotation of the arm, *i. e.*, the elbow joint.

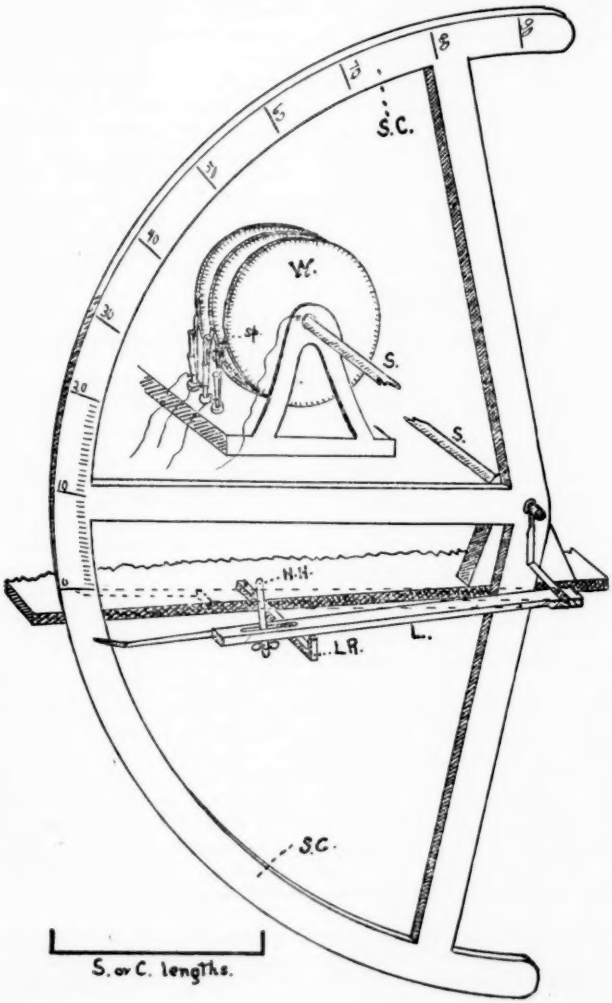
2. On this shaft a wheel (*W.*) of non-conducting material (fibre) is fixed, bearing across its edge metallic blades at equal distance from each other. These blades are connected with the axis of the wheel, and the axis with an ordinary recording magnet with which is also connected a spring (*sp*) pressing against the edge of the wheel. Thus an electric circuit is completed whenever one of the blades passes under the spring and recorded on a kymograph drum.

As an ordinary recording magnet cannot be made to move at a greater speed than 100 movements a second, several wheels may be used. If three wheels, each connected with a separate recording magnet, be used and the distance between the blades on the rims of each wheel be 2 degrees, movements of any velocity, up to 450° a second (that is, $\frac{2}{3} \times 300$), can be recorded.

The wheels are to be adjusted on the axis so that they produce contacts successively and not simultaneously.

The distance between each two successive downward move-

ments made by each one of the recording magnets corresponds to an arm movement of 2°. But by combining the lines traced



by the three markers, the distance of two successive movements corresponds to ⅔ of a degree. Thus the length of the arm movements, wherever made on the semi-circle, can be computed on the kymograph records. If a time line be added, the total duration of the movement as well as its velocity at any point can be read.

This method of determining the length and the duration of a movement is evidently subject to an error, an underestimation: the movements begin to be recorded only when the first contact is made, and cease to be registered when the last contact takes place, although the movement usually begins a little before and continues a little after. But the average amount of this underestimation, depending as it does upon the distance of the blades from each other, is easily calculated. Moreover, this error affects equally, on the average, the standard and the comparison movements. It can, therefore, be looked upon as immaterial, at least when the movements have a certain length and when the number of tests is sufficiently large.

It should not be overlooked, however, that one is not dependent upon the drum records for a knowledge of the length of the movements. Except when the movement is a free-ending one, the lengths of both the standard and the comparison are determined by the operator himself. And as to the free-ending comparisons, their lengths can be read directly on the semi-circle (*S. C.*).

The duration error mentioned above can easily enough be done away with by discarding the wheels, connecting the end of the lever (*L.*) with the recording apparatus, and using metallic standard and comparison lengths (*S.* and *C.* lengths) also connected with the recording magnets. These lengths may be made of sheet metal bent at both ends and placed wherever wanted on the semi-circle. In the initial position the point of the lever is in contact with one of the bent ends and in the final position, with the other. Thus an electric contact is broken at the beginning of the Standard and of the Comparison and one is made as they are completed. This method, used by us in several series, has its advantages and its drawbacks. It cannot, of course, be used when the comparison is to be a free-ending movement. And the click produced by the end of the lever striking the ends of the metallic lengths is an undesirable complication.

This apparatus meets, to some extent at least, a need long felt in the study of kinæsthetic space perception by means of arm and leg movements. Its chief uses and advantages are as follows:

1. The movements are arc movements, *i. e.*, movements involving but one joint and the muscles operating it.

2. It is usable for movements of the forearm only, or of the whole arm, of any length and at any point of the arc the arm can describe. The accompanying drawing represents the apparatus set up for the study of vertical movements; but it can be set up for the investigation of movements in the horizontal plane.

3. It can be used for active and for passive movements, and also for the study of the influence of resistance. For this last purpose, a weight is attached to a wire running in a groove made in the rim of one of the wheels.

4. In the case of movements of the fore-arm in the vertical plane, the upper arm hangs freely from the shoulder, and thus disturbing strain elements, present in most of the other methods, are eliminated.

5. It does way with the sensation-complications produced by holding a pencil and drawing a line, or by marking the beginning and the end of the lengths by a pressure. For it was found easy for the observer to maintain practically the same pressure on the hand-holder (*H. H.*) from the warning signal to the return to the rest position. It may be preferable, however, to tie the arm to the lever, and thus do away with the possibility of changes in the hand pressure synchronous with the movements compared.

6. Used as we have used it, it is soundless. The only parts of the apparatus which make any noise are the wheels (*W.*) in rubbing against the springs (*sp*) and the recording instruments. In our experiments these were placed in a room adjacent to the one occupied by the observer.

7. The elimination of the sensations arising from contact of the air with the skin of the arm and hand, is secured by the use of a loose glove which reaches nearly to the elbow where it is covered by a loose sleeve. This, together with the elimination of sound and of the other disturbing sensations already referred to, leaves as factors to be considered practically only the joint, the tendon, and the muscle sensations. The direction of the attention to one or the other of these is thus made considerably easier.

8. It provides for the recording of the duration of movements of any length, performed at any point of the arc described by the arm, in both directions. It may thus be used for an exact study of the duration and of the velocity factors in the estimation of spatial arm movements. Free movements are recorded and timed as well as those that are objectively limited.

9. The apparatus can easily be adapted to the study of leg movements.

THE INFLUENCE OF THE DURATION AND OF THE RATE OF ARM MOVEMENTS UPON THE JUDGMENT OF THEIR LENGTH

By PROF. J. H. LEUBA, assisted by MISS ETHEL CHAMBERLAIN, Fellow in Psychology, Bryn Mawr College

Description of the Experiments. During the past year I undertook, with the help of the apparatus described in the preceding pages under the name of "An Apparatus for the Study of Kinæsthetic Space Perception", a three-part investigation of the duration and of the velocity factors in the estimation of the length of arm movements made by the forearm. Two of these parts are still unfinished. They deal, one with the source of the constant error arising when a length, objectively limited, is reproduced by a free, *i. e.*, unlimited, movement; the other with the effect of voluntary changes in the velocity with which the reproducing movement is made.

The part here described consists of four series: 1. a Normal Series (N. Ser.)[1]; 2. a Weight Resistance Series (W. R. Ser.)[1]; 3. a Muscle Resistance Series (M. R. Ser.)[1]; 4. a Control Series, or an Increasing Weight Resistance Series (I. W. R. Ser.)[1].

The method used throughout was the modification of the Method of Average Error called the Method of Constant Stimuli. The observer's task in this method is to compare a standard movement with a number of fixed comparison-lengths so chosen that the smallest seems distinctly shorter and the largest clearly larger than the standard (St.)[1], and to express his judgment by one of the words, "smaller", "equal", "greater". I have used only three comparison lengths (C.)[1]. The results show this small number to have been sufficient for our purpose.

In each of the four series, the St. Movement (Mt. in the singular and Mts. in the plural)[1] was a length of 8° gone through in the upward direction and beginning each time at a different point between 0° and 5°. The zero point is the one indicated by the pointer when the arm is held horizontally. The reason for this latitude in the starting point of the St., and for a similar latitude in the starting point of the C. was

[1] These abbreviations will be used throughout the article.

the wish to make judgments by means of the sense of position impossible.

In the N. and in the W. R. Ser., the C. Mts. began between 25° and 30°, *i. e.*, approximately midway between the position of the St. in all Ser. and that of the C. in the M. R. Ser.

In the N. Ser. the Mt. was unimpeded; in the W. R. Ser. a resistance of 1½ kilogr. was provided in the form of a weight attached to a wire running in a groove made in the rim of one of the wheels, the whole being so disposed that the weight was put on only after the St. and before the C. Mt.

In the M. R. Ser., the C. lengths were so placed on the semi-circle as to end at different points within 3° of a point 1° from the extreme position the arm was able to reach. A rest was provided in this series for the back and the head of the subject so that his position with regard to the apparatus would remain constant. A very considerable resistance, due to the pressure of the forearm against the biceps, was thus introduced. It is to be noted that this resistance was not constant from the beginning to the end of the C. Mt., as it was the case in the W. R. Ser., but that it increased steadily until its conclusion.

Two subjects performed the four Ser., one of them (R.), a graduate student in psychology, the other (L.), the writer who was the only subject informed regarding the purpose of the experiment and the detail of procedure. A third subject (H.), a senior student, who had already had a good deal of experience in the comparison of arm Mts., served for the W. R. and the M. R. Ser.

The observers worked with their eyes bandaged, except L. who kept them closed without that help. Practice series were given before each new Ser. and a few tests at the beginning of every session. These sessions were limited to about half an hour's duration, broken by several interruptions for rest.

Introspection was asked for only towards the end of each series.

The directions given to the subjects were simply that they concentrate their attention upon the length of the Mts. and express by the words, "shorter", "equal", and "longer", the relation of the C. to the St. The words "duration" and "velocity" were not mentioned to them, at least not before the introspective account was asked for. They were thus left to move at the speed they liked best.

Results. I. I give first, as a sample, a complete record of the N. Ser. of L. (Table I). The figures indicate, in terms of fiftieths of a second, the duration of the St. and of the C. Mts. when the C. were respectively 7°, 8°.5, and 10° and also the judgment passed by the subject in each case. Observe the

TABLE I

Normal Series

St. = 8° Subject L. Equality judgment 8°.4

C. = 7°						C = 8°.5						C. = 10°.					
—		=		+		—		=		+		—		=		+	
St.	C.	St.	C.	St.	C.	St.	C.	St.	C.	St.	C.	St.	C.	St.	C.	St.	C.
67	85	66	88			73	80	75	84	82	87			83	101	79	93
84	72	83	72			74	58	81	88	80	91			71	97	97	95
63	82	119	68			78	62	81	90	67	88			111	88	73	106
85	57	77	42			63	68	77	76					86	88	88	102
60	65	63	48			79	56	94	77					58	66	112	101
83	67	86	81			73	58	86	93							69	90
60	49					79	77	75	69							82	113
75	54					69	83	75	82							86	109
58	52							95	65							76	94
87	69							85	75							53	94
83	59							76	69							73	101
79	62							47	64							73	101
92	59							55	55							70	64
74	68							63	52							69	74
74	61							60	79							70	80
67	66							106	92							70	85
90	50															74	81
71	43															77	95
88	—															59	78
87	55															71	72
74	46															79	85
																64	94
SUMS 1601	1221					588	542	1231	1210							1664	1967
AVH. 76	61					73·5	68	77	76							76	89

Average of all — judgments, St = 75, C = 61, Differ. = — 14.
" " " = " St = 79, C = 76, Differ. = — 3.
" " " + " St = 76, C = 90, Differ. = + 14.

considerable variable error in the duration both of the St. and of the C. In column 7°—, for instance, the St. times vary from 58 to 92. The differences between the compared St. and C. are also considerable. They range in the same column from +19 to —40. Similar variations were found in series of experiments in which the observer was comparing not the lengths of Mts., but the *duration* of pressures upon his fore-finger. I shall use this similarity in a subsequent article to reinforce the thesis that in the estimation of the length of Mts. here dealt with, it is the duration and the velocity, not the length, which are directly perceived. Nevertheless, the average duration of the St., leaving out the columns containing

too few tests, is strikingly uniform: 76 for column 7°—; 77 for column 8°.5=, and 76 for column 10°+. We look upon this uniformity as an indication of the sufficiency of the number of tests taken.

The duration of the individual Mts. varies for L. in this series from a little below one second to a little over two seconds. He moved more rapidly in the M. R. and the I. W. R. Ser.

The most interesting figures of the table are those expressing the duration of the C. When the C. was 8°.5 in length and felt equal to the St., St. and C. were made practically in identical times (77 to 76); when the same C. was felt shorter, its duration was materially less than that of the St. (73.5 to 68). If we pass to the comparison of the St. with a C. length of 10° (felt markedly longer), we find that it took a much longer time to make the C (76 to 89). The proportion existing in this case between the duration of the compared movements is practically equal to that existing between their lengths: $\frac{76+0.5}{90}=\frac{8.5}{10}$ When the C. Mt. was 7°, it was made in considerably less time than the St. (76 and 61). The other columns contain too few tests to yield an average free from the variable time error to which I have drawn attention.

These figures lead, it seems, to the conclusion—a conclusion to be reinforced by our other results—that in comparing, as he thinks, two lengths, L. compares in reality, the duration of two movements made at approximately the same rate. I may add that during the experiment he did not have duration in mind. He strove to reproduce in the C. the sensations of movement experienced in the St. The sensations that drew his attention seemed to be localized in and around the elbow joint.

II. In order to get the duration figures given in Table II, the sums (not the averages) of the —, the =, and the + columns in each series were added, and the total divided by the number of tests included in each column. The differences between the St. and the C. are indicated in parenthesis. The W. R. Ser. of H. includes but 50 tests; of these only ten fall in the + column, hardly a sufficient number for a reliable average. The figure in the equality C. (marked with an asterisk) is not strictly comparable with the others. It is derived from a series not reported in this paper in which the C. Mts. were made considerably higher up on the semi-circle than in the N. Ser. The figure may serve, however, as an approximation.

If the judgments recorded in this table are based, as it appeared probable from the series already examined, upon time

TABLE II

Average duration of all the tests arranged by series and classified according as the C. was judged larger, equal, or smaller than the St.

Subject	Series	—			=			+			Equality C.
		St.	Diff.	C.	St.	Diff.	C.	St.	Diff.	C.	
L	N	75	(—14)	61	79	(— 3)	76	76	(+14)	90	8°.4
	W R	76	(—10)	66	78	(+ 1)	79	80	(+11)	91	8°.3
	M R	Not enough cases. (6)			48	(— 3)	45	54	(+ 2)	56	7°.3
	I W R	50	(— 1)	49	51	(0)	51	50	(+ 9)	59	7°.9
R	N	39	(+ 2)	41	37	(+ 5)	42	37	(+ 9)	46	8.7
	W R	41	(+ 8)	49	40	(+11)	51	41	(+19)	60	8.5
	M R	32	(+ 6)	38	31	(+ 8)	39	31	(+14)	45	7.5
	I W R	43	(+ 5)	48	40	(+ 7)	47	40	(+13)	53	7.6
H	N										10*
	W R	30	(+ 6)	36	29	(+11)	40	31	(+10)10 cases.	40	9.8
	M R	29	(+11)	40	29	(+15)	44	31	(+16)	47	7.2

comparisons, we should expect the duration of the compared Mts., in every case in which they are judged of equal length, to bear to each other a fixed relation, whatever be the length of the C. That fixed relation would be one of equality if the subject moved through the C. with the same velocity as through the St. But if he should move systematically slower through one of them, the constant relation could not be one of equality. I assume, of course, in saying this, that the subject is influenced, not only by the duration but also by the speed of his Mts., that, in fact, a quasi-automatic compensation takes place between duration and speed when he is judging of spatial lengths.

The data before us bear out these suppositions. Subject L., moving through the St. and the C. in the N. and in the W. R. Ser. at an approximately equal velocity, finds the C. equal to the St. when both take the same time, shorter than the St. when the C. takes less time, and longer than the St. when it is the reverse. In the M. R. and the I. W. R. Ser., done several days after the others, he again maintains practically the same rate of speed through the St. and the C. Mts., although the absolute speed is considerably increased when compared with that of the first two Ser. Subject R. differs from L. in that she takes the C. more deliberately than the St., and, allowing for the lesser speed of the C. Mts. calls, in the first series, the C. equal to the St. when the former takes her 5 units longer. When it takes her only 2 units longer, she calls it shorter, and

when it lasts 9 units more, she calls it longer. In the W. R. Ser. she proceeds through the C. still more slowly than in the preceding Ser., and the figures show that, even though she is not clearly conscious of it, this decrease is taken into account. Her Equality C. takes her 11 units longer than the St. When the C. takes her only 8 units longer, she calls it shorter, and when it lasts 19 units more than the St., she calls it longer. Her last two Ser. are, in this respect, in full agreement with her first two Ser. The averages from H., as far as they proceed from a sufficiently large number of tests, match those of the two other subjects.

We find, thus, conclusive evidence that these subjects, set to the task of comparing the length of two arc movements, were guided in their judgments by the duration and the rate of the Mts., although their introspection does not indicate any awareness of the fact. The degree of precision with which changes of absolute speed and of relative speed between the St. and C. are taken into account, came to me as a surprise. And the further fact that, making use of duration, the subjects did not try to keep the velocity equal (L. excepted) was, I confess, somewhat disconcerting. I intend to find out at some future time whether persons required to compare the length of two Mts. by means of their duration would not endeavor to move with an equal speed through both.

Explanation. Let us, now, compare in the different series the average lengths of the C. when it was felt equal to the St. of 8°. These lengths are written down in the last vertical column under the head "Equality C." With regard to the influence of the weight resistance, they confirm, in every subject, the results obtained by other experimenters, *i. e.*, the equality C. in the N. and in the W. R. Ser. do not differ materially one from the other. It should not be overlooked that it is the addition of a *constant* resistance which does not alter the length of the Mt. The *increasing* resistance used in the M. R. and in the I. W. R. Ser. produced a substantial overestimation of the C. Mts. But why this overestimation of the C. in the M. R. Ser.? We know from the figures of Table II that it has not its origin in an error of duration: a shorter time is not mistakenly judged equal to a longer one; the lengths 7°.3 and 8° felt equal by L. are made in equal times. If not in the duration, the error must be in the estimation of the rate of Mt. There is as a matter of fact in the M. R. Ser. a gradual reduction in the velocity with which the arm moves through the C. This falling off is correlated with the increasing resistance offered to the Mt. by the forearm pressing against the upper arm. We shall submit figures on

this point in a future paper. But the falling off of the rate is not in itself sufficient to cause an error since, as we have seen, changes in velocity are, under certain conditions, exactly taken into account. It must be that in this Ser. the conditions do not allow of the normal compensatory effect of the speed upon the duration.

At this point we must inquire into the sensory basis for the apprehension of the rate of Mt. The skin sensations,—air pressure and temperature—at best secondary criteria, had been eliminated in the cases of R. and H., by means of a loose fitting glove, reaching almost to the elbow, and overlapped by the sleeve. These sensations being out of the question, there remained those arising from the muscles, from the tendons, and from the joint. When the arm is moved through a fixed number of degrees, the effort made is greater when the velocity is high than when it is low, because a part of the innervation finds its way into the antagonistic muscles, and an increased innervation of the muscles that do the work is required. But I do not see any reason for assuming that, as far as tendinous and muscle sensations are concerned, the untrammelled arm moving upward at a considerable speed yields a sensation-experience different from that produced by a slower movement of the same duration made against a properly chosen resistance. The muscle activity would, it seems, be the same in both cases. The muscle and tendinous sensations cannot, therefore, provide an unequivocal basis for the rate of Mt. One is, thus, driven to the hypothesis that unequivocal speed information comes from the joint surfaces.

What is it, then, that takes place in the joint surfaces when speed changes? Two things: (1), an increase in the pressure of the joint surfaces against each other, corresponding to the greater innervation of the muscles. This pressure-increase produces, presumably, a rise in the intensity of the joint sensations. But an intensity series correlated with an increase in muscular tension cannot, as we have seen, serve as a basis for an apprehension of speed. (2.) Speed-increase means also an increment in the rapidity with which the joint surfaces pass over each other, *i. e.*, an increase, shall I say, in the quantity of the stimulus applied to each one of the joint sense-organs in a unit of time? It is credible enough that a qualitative or a quantitative sensation-difference corresponds to this quantitative increase of the stimulus. We may think of it as being the same sort of difference as the one experienced when the velocity of a wheel, the edge of which rubs against the skin, is altered. Whatever be the psychical effect produced, in the case of the joint organs, by changes in the rate with which the stimuli succeed each other, that seems clearly

the only possible cause of a sensory change corresponding unequivocally to the rate of the Mt.

I shall therefore hold, until further information comes to hand, that our comparative judgments of the length of arm Mts., when the "sense of position" is excluded, is really a comparison of duration and of a peculiar sensory value depending upon the rapidity with which successive joint organs are stimulated. This sensory value may be called the *rate-value* of the joint sensation. That introspection does not clearly reveal the existence of this rate-value is no argument against its effective presence.

We are now prepared to account for the over-estimation of the rate of Mt. in the M. R. Ser, an over-estimation leading to the length errors recorded in Table II. I have said already that as the C. movement proceeds the resistance increases somewhat irregularly and the velocity falls off. It decreases, not for the physiological reason offered by Loeb, but because the effort made at any particular moment to overcome the increased resistance, so as to keep the speed constant, falls short of its purpose, since by the time the effort becomes effective the resistance has again increased. The decrease in speed would not of itself cause the observed error. It is because of complicating circumstances making the correct estimation of the rate impossible that the Mt. is made shorter. What happens is that the rate-value of the joint sensation is obscured by a gradual and somewhat irregular increase in the intensity of the joint sensations,—an increase arising from the increment in muscular tension made necessary by the growing resistance. Furthermore, and chiefly, the necessity of repeated readjustments of the speed to the resistance tends to draw the attention away from the rate-value.

The error is an *overestimation* of the rate because the intended speed is greater than the realized one.

In the W. R. Ser. the disturbing circumstances just mentioned did not exist. There was, indeed, an increase of the muscle tension in the C. Mts., and, presumably, a slight corresponding increment in the intensity of the joint sensations. But this increase was, in this series, constant from the beginning to the end of the C. movement. It did not, therefore, divert the attention from the rate-value and thus no velocity error took place.

If the duration of the Mt. remains undisturbed in the M. R. Ser., it is, I think, because the duration of the Mt. experience as a whole, or of the joint sensations in their entirety, is estimated by means of the duration of other sensations themselves unaffected by the arm movement. It is, therefore, an indifferent matter whether or not any particular qualitative or inten-

sive alteration takes place in any one of the sensory components of the whole Mt. experience. The important points for a correct duration-estimate are that the Mt. should be present in some way in consciousness and that the sensations used to measure its duration—and we know that they are not necessarily the same for every one—remain undisturbed by extraneous factors as long as the Mt. lasts.

In order to verify the preceding explanation of the space error present in the M. R. Ser., a control Ser. was devised. In it the C. Mts. took place over the same portion of the semicircle as in the W. R. and the N. series. A resistance was introduced in such a way that, instead of being constant as in the W. R. Ser., it increased as the C. Mt. proceeded. Thus the pressure of the forearm against the upper arm, and the high degree of contraction of the biceps were eliminated, but the increasing resistance remained. Under these circumstances one would expect, if our explanation is valid, a rate error just as in the M. R. Ser. The resistance provided was a long weight of 2 Kgr. dipping in mercury. During the C. Mt. the weight was gradually lifted out of the mercury. As, according to our practice, the starting point of the C. as well as of the St. varied by a few degrees in this series also, the initial resistance in the C. Mt. varied from 0 to about 300 grams. The amount of resistance at the end of the Mt. was not at all as great as in the M. R. Ser.; nevertheless, the results show, in the case of R., an overestimation of the equality C. practically equal to the one present in the M. R. Ser. (7°.6 against 7°.5 in the M. R. Ser.). The error is not so great in the case of L. (7°.9 against 7°.3 in the M. R. Ser.), yet an unmistakable error in the same direction as in the M. R. Ser. is apparent in his results. It is in fact the size of R.'s error and not that of L. which is surprising in view of the considerably smaller resistance applied in this Control Ser. Lack of time prevented H. from doing this series.

Our account of the over-estimation of the equality C. is thus verified, as far as it was in the power of the Control Series to do.

Historical and Critical. In 1890 J. Loeb published in Vol. 46 of *Pflüger's Archiv*, pp. 1-46, under the title, "Untersuchungen über die Orientirung im Fühlraum der Hand und in Blickraum", certain experiments with arm movements from which he concluded that the more contracted are the active muscles at the beginning of a movement, the greater is the overestimation (p. 41). In order to account for this fact he makes use of two hypotheses, the first of which is superfluous, and the second (dependent upon the innervation theory) is now quite discredited. They are, (1.) the excitability of a muscle decreases as its state of contraction increases; (2.) the

length of a Mt. is judged by the amount of energy sent to the muscles performing it. If, then, we try to make two equal Mts., the second of which follows in the direction of the first and is therefore performed with the muscles in a greater state of contraction, we shall send to the muscles for the execution of the second movement an amount of energy equal to the amount by which the first was produced. But as the more contracted muscles are less excitable than the less contracted ones, an equal amount of energy will produce a shorter length in the second than in the first Mt.

F. Kramer and G. Moskiewicz in "Beiträge zur Lehre von den Lage- und Bewegungsempfindungen" in the *Zeits. f. Psy.*, Vol. XXV (1901), pp. 101-125, repeated Loeb's experiments and, generalizing his conclusions, stated that of two movements intended to be of equal length, everything else remaining equal, the more uncomfortable (*unbequem*) falls short of the other. They rejected Loeb's explanation, and suggested, without putting their supposition to an experimental test, that the ovestimation of the "*unbequem*" Mts. is the outcome of a natural tendency to move more slowly through a difficult Mt. than through an easy one (pp. 121-123). This statement we know to be inexact, unless the effect of an added resistance, as in our W. R. Ser. does not fall within the intended meaning of the word "unbequem".

The assumption of Kramer and Moskiewicz with regard to Loeb's illusion was recently put to a test by Erich Jaensch in "Ueber die Beziehungen von Zeitschätzung und Bewegungsempfindungen", *Zeits. f. Psy.*, Vol. 41 (1906), pp. 257-279. He used for measuring the duration of the Mts. a pencil so constructed that when its point is pressed down upon the paper it recedes into the handle. Air is thus pressed back through a rubber tube into a Marey's tambour. In this way a record is made of the beginning and of the end of the Mts. In one of these two series of experiments the observer, starting with the hand near the chest, moved it away from the body for a certain distance, stopped an instant, and then proceeded in the same direction until he had made a line seemingly equal to the first. In the other set, the direction of the Mts. was reversed, the observer starting away from the body and coming toward it.

His duration records led him to this conclusion: "One may take it as proven that the lengths appear equal because the times used in making them are equal" (pp. 269). He discusses the causes of the lower velocity of the uncomfortable Mts. and comes to the opinion, different from ours, that the chief one is physiological, namely, the decreasing excitability of the contracting muscle.

Loeb's explanation of the decrease in length of the Mt. is

thus taken up by Jaensch as the cause of the decrease in its rapidity. This decreasing excitability hypothesis is shown by our Increasing Weight R. Ser. to be superfluous. Moreover, if the relation it supposes between the degree of contraction and excitability really existed, we would in all likelihood have learned to increase automatically the innervation as the length of the muscle decreases and thus have overcome the tendency to error. Jaensch does not seem to have clearly realized, any more than his predecessors, what our experiments prove, namely, that it is an *increasing resistance* and not an added, constant resistance which causes the velocity error in Loeb's experiments. Neither did he grapple with the real problem, which was to make clear why, while we are usually able to take into account, for the sake of the length of the Mt., minute changes of velocity, under special conditions, a constant error in the estimation of speed takes place. To this problem an answer has been given in the preceding pages.

Introspection revealed that our three subjects made no use of visual imagery. In the case of L. there was at times a vague image of the space covered, but it followed the production of the Mt. and never had the clearness necessary in order to serve as a guide. One of the two subjects we used in other experimental series, and only one, had clear visual images of the length of the Mt. She saw the St. and tried to reproduce an equal visual length. Of all our subjects she was the least accurate. I hope to have the opportunity of finding out whether she also made use of duration and rate in judging of the length of Mts.

In the light of the preceding facts and discussions the complete inadequacy of Külpe's statement is evident: "our judgment of the extent of arm movement is not based (in the author's observation) upon the temporal relations of the movement, but upon the reproduced visual image of the space passed through, and more especially of the extreme positions of the moved arm." (Outlines, p. 348.)

Before closing I must refer to the constant positive error in the Equality C. of the N. and of the W. R. Ser. in the case of the three subjects. (See the last column of Table II.) I have so far neglected it. My reason for doing so is that I have at present nothing decisive to say about it, I do not know how to account for the overestimation of the velocity, that, in my view, it implies. There are several possibilities. I shall probably return to the point in another paper.

Summary of Conclusions. When the sense of position is ex-

cluded, the comparison of the length of arc movements is made through the comparison of the duration of one or several of the sensations arising from the Mts. (preferably the joint sensations) and of a particular value of the joint sensation, called here the rate value.

A quasi-automatic compensatory relation exists between the duration and the rate value.

A gradual increase in the resistance offered to a Mt., whether caused by a weight or by the pressure of the forearm against the upper arm, produces a decrease in the rapidity of the Mt. This decrease, for reasons mentioned above, is underestimated, and thus an overestimation of the length of the movement takes place.

Local signatures cannot be connected with the rate value of joint sensations, for one and the same joint organ is susceptible of a whole range of rate values. It is the joint sensation as a whole, not the rate value, which possesses, or may possess, a local sign.

The comparison of the length of Mts. under the conditions present in our experiments does not necessitate the existence of local signs in the joint sensations. An apprehension of duration and of rate is sufficient.

ORGANIC SENSATION[1]

By ELSIE MURRAY

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[1] The investigation of which this article forms a part was first taken up in the Cornell Laboratory in the year 1905-6. A related article appeared in this *Journal* in July, 1908.

It was our original intention to limit this study to the sensations originating in the vegetative processes of the visceral and circulatory systems, exclusive of all cutaneous or kinæsthetic components. In the course of investigation, however, the impracticability of keeping up this line of division became apparent, and the discussion is frequently broadened to cover *internally or indirectly initiated* sensation in general, a category practically identical with what Sherrington has termed the 'proprio-ceptive', as over against the 'extero-ceptive', sphere of sensation. The reasons for this will be more apparent in a discussion of the subject which is to be published later.

Preliminary Survey

The following study represents an attempt to bring a subject commonly assumed to be of physiological import only under the discipline and into relation with the data of experimental psychology. In view of the scope and novelty of the undertaking and the inherent difficulties of the subject, apology need hardly be offered for the fact that in the following pages project rather than accomplishment is presented to the reader.

To the rôle of somatic sensation in our daily consciousness various set descriptive phrases, such as the 'thrill of pleasure', the 'shock of surprise', the 'pang of dismay', the 'wave of pity', the 'shudder of disgust', bear witness. That these phrases are not merely metaphorical in significance, but have a direct physiological reference and derivation, that the 'thrill', the 'pang', the 'wave', has each its own more or less definite bodily seat, characteristic timbre, and limited emotional context is sufficiently patent to all. From the literature and speech of every day many phrases referring directly to heart or circulatory sensations may also be culled, *e. g.*, the heart 'aches', 'sinks', is 'ready to burst', the blood 'boils', 'curdles', the gorge 'rises', and the like. To this list other less highly colored yet distinctive and persistent bodily reactions, fatigue, hunger, tension, excitement, satisfaction, and many more or less shadowy modes of cœnæsthesia may be added. Of the group thus constituted it may be safely said that in some form or other, in the focus or margin of consciousness, one or another of them is always with us, even in the most intellectual of our preoccupations. That these, the most intimate, unescapable, and superficially accessible of our sensory experiences should stand to-day on the frontier as it were of the psychology of the senses is an interesting if hardly creditable phenomenon.[1] Speculation as to the significance and inner mechanism of certain of the bodily accompaniments of our mental states or emotions has indeed been rife, witness the James-Lange theory and the present discussion pro and con. But of curiosity as to the primary sense stuff of these bodily reactions, of query whether the diversity in the varying web of mood is one of 'color' or texture merely, of inquiry as to the possible existence of an extended gamut of organic as of visual sensation, of exploration of the peculiar cohesions, conformations, and behavior of such sensations in experience, there is in the litera-

[1] See E. B. Titchener: The Problems of Experimental Psychology (St. Louis Congress address), this *Journal*, xvi, 1905, 212. "Of all problems in the psychology of sense that are now before us, the problem of the number, nature and laws of connection of the organic sensations appears to me to be the most pressing."

ture singularly little evidence. Yet if psychology is to be raised above the level of the merely subjective, any explanatory use of organic sensation must of necessity be preceded by some sifting of the facts and agreement as to the states in question. Here, if anywhere, are surely in order the old-fashioned, plodding methods which seek by the correlation of fixed terms with certain of the simpler, commoner, and more readily identifiable phases of consciousness to pave the way for the description and exploitation of the subtler and more significant phases within which the simpler function as partials, or prime factors.

The following research was prompted in part by a desire to bridge the gap in our knowledge of the psychology of the sensory components of experience. This motive was, however, strongly reinforced by the conviction that (all question of their specific function in the vegetative economy of the organism aside) these so-called 'by-products' of the mental life must play some considerable rôle not only as the matrix or basis of continuity of our experience, but further as conditioning in some fashion both the affective processes, and attention, action, and the interplay of ideas and meanings. Inquiry was further stimulated as the result of certain observations incidental to a laboratory investigation of the affective qualities, in which the writer, serving as observer, had occasion to note that even the simple pleasantness (or unpleasantness) occasioned by various sensory stimuli was attended in consciousness by certain more or less definite and uniform if hardly describable bodily reverberations or feelings. In view of the difficulties in the way of unequivocal correlation of affective qualities with objectively demonstrable organic changes,[1] the possibility of correlation with *felt* organic changes (such as the bodily set or mood of acceptance or rejection) is not to be let slip unchallenged. Without prejudice to the question whether or not the organic sensations form the indispensable basis and vehicle of affective experience, as d'Allonnes would have us believe, the demonstration of certain constant correlations between affection and bodily sensation would at least afford a tangible basis for the handling of affective experiences, for their release from the disconcertingly detached and disenfranchised position which they now hold in psychology.

However this may be, it is not unreasonable to surmise that the shirking of the analysis of the bulky if nebulous residue of our marginal consciousness constituted by organic sensation

[1] See, *e. g.*, R. Lagerborg's résumé of the discussion as to the symptomatic or causal relation of organic changes and feeling, in Das Gefühlsproblem, 1905, 42-68.

plays into the hands of an uncritical use of the 'subconscious' in explanation, and of a more or less reckless speculation as to the number and traits of the affective elements, the ultimate constitution of mind, and the functions and attitudes of the empirical (and non-empirical) ego. In view of the present unsettled state of the science, and the growing demand that psychological treatment should render itself more adequate to concrete reality, scientific economy forbids the leaving at loose ends of so large a body of conscious fact. In the attempt to establish a descriptive basis for the psychological treatment of functions and attitudes this study must inevitably contribute by bringing into the light of day many supposedly intangible and shadowy modes of experience.[1]

Again, in view of the more or less prevalent lack of recognition of any simple affective consciousness other than pleasantness or unpleasantness attaching to our ordinary perceptual experiences, and the consequent reproach of over-intellectualism, the possibility that the organic coloring of conscious events may serve not only to enrich the affective consciousness constituted by the above mentioned elements, but may itself represent an independent feeling moment fairly on a par with pleasantness and unpleasantness, deserves examination. Whether or not this speculation prove profitable, introspective examination of bodily sensation under experimental conditions should be employed to establish the present debatable boundary line between organic sensations or complexes and unitary affective processes, pleasantness ,and unpleasantness, or tension relaxation, quiescence, and the like, in the systems of Ladd, Wundt, and Royce.

Lastly, whatever may be the relation which subsists between feeling and organic sensation, in the opinion of the writer discussion of the James-Lange theory, and experimentation with animals and abnormal subjects in support and in disproof of it, are in a sense both premature and futile so long as the method of introspection has not been stretched to the uttermost and the possible organic contributions to emotion, and their behavior in simpler and more complex contexts critically examined.

[1] With an organic terminology at our command we shall be able to do fuller justice to the reactive as well as the presentative aspects of perception and thought. It is indeed the hope of the writer, as doubtless of many others, that a study of the organic margin or lower stratum of everyday consciousness will not only ultimately contribute to the elucidation of such refractory psychoses as the judgment of certainty, of likeness, the feeling of reality, of contrast, of recognition, but may even furnish a basis of distinction for peripherally and centrally excited elements, for recollection and imagination, as well as for many of the so-called 'feelings of relation'.

Primary Problems of Organic Sensation

Certain fundamental questions as to the nature and conditions of appearance of organic sensation in contradistinction to recognized sensory qualities loom large at the very outset of our undertaking. Is, for instance, the customary dichotomy of sensation into external and internal psychologically valid? Does it have its roots in a fundamental disparity between the underlying psychophysical conditions or is it an epistemological distinction merely? Does it have an ultimate justification if not in elemental qualitative differences, at least in the diverse structural or textural patterns and cohesions to which the two classes of sensation are liable, or in the extent to which they are amenable to analysis and reproduction? The sources to which we may apply for aid in the solution of these problems are various. Casual references to organic sensation in literature in general; in monographs dealing with the problem of feeling in especial; the relevant facts of histology, physiology, and pathology; and, lastly, direct experimentation and introspection, must all be drawn upon to contribute what light they may to the subject. Preliminary to more detailed discussion, the general trend of opinion of psychologists and physiologists on three or four elementary phases of the problems involved will be briefly sketched. The neurological, physiological, and pathological evidence bearing on these same problems will then be more minutely examined.

1. *Possibility of observation in the sphere of organic sensation.* In explanation of the dearth of systematic observations in this field many difficulties have been cited; the inaccessibility alike to experimentation and attention of the sensations concerned, their alternately feeble and violent character, high affective tone, unanalyzability, and vague sensory coloring.[1] With re-

[1] The problem is somewhat obscured by the custom deriving from Weber, Wundt, and Goldscheider of lumping organic or visceral sensation with the *Gemeinempfindungen* ('common sensation'), a class which by definition consists in whatever residue of sense material proves resistant to elaboration, analysis, and localization. The significance attributed to this classification varies considerably in the different writers, and may be quoted here in view of its bearing on their several attitudes toward organic sensation proper. The term or its equivalent is used in turn to cover sensation derivable from any portion of the body or any nerve, sensation referred to the body (as a whole), or totally unreferred, subjective. In the usuage of J. Mueller the significance of the adjective 'common' is anatomical or topographical merely: common sensation (*Gemeinempfindungen*) includes the pain-pressure-temperature equipment common in his belief to the exterior and interior of the body alike. Weber, using the term *Gemeingefühl* or *Gemeingefühlsempfindung*, gives it an epistemological turn: common sensation is unexternalized sensation, the raw sense material in the formal elaboration of which and the application of spatial, in-

gard to the impracticability of experimentation, a word only is necessary. This difficulty first finds expression with Weber, who in listing the reasons why our inner sensations are customarily outside the range of apperception, gives prominence to the fact that our inner organs possess no *Ortsinn*, and are withdrawn from the possibility of the local application to their surfaces of moving or changing stimuli, hence can never take their place in the world of outwardly referred, spatially determined perceptions.[1] For this formulation of the difficulty the Kantian principle adopted by Weber that only the *spatially* determined and outwardly projected[2] attains to any degree of

tensive, or qualitative categories to which perception is balked, either by the violence of the sensation itself or the conditions of its origin (internal stimulation). This rubric, which includes tickle, itch, organic sensation and pain, is stretched to cover muscle sensation also, but obviously not without violence to the original meaning. Practically the same subdivision is set up by Wundt on the basis of the internal nature of the stimulus (functional changes, central irradiation, etc.) as a condition unfavorable to projection. Wundt, however, sees the impossibility of retaining kinæsthetic sensations in the category, though still including pain, which certainly may appear in as objective a light as strain. Goldscheider, using the term *Gemeingefühl*, shows a tendency to apply it to organic and external sensation only in so far as these are actually unanalyzed and unprojected, *i. e.*, felt rather than perceived. Thus a faint tactual impression may consist either in a touch perception, a tickle feeling, or both, according to the conditions under which it appears. Kröner, on the other hand, insists upon the limitation of the term *Gemeingefühl* to its strict logical signification, *i. e.*, a sense consciousness absolutely unanalyzable (owing to the central or general nature of the stimulus), and referred to the body as a whole. Hunger, thirst, drowsiness, nausea, general bodily comfort or discomfort, and various other organic states, belong under this heading. Lagerborg makes a somewhat similar use of the category of *Gefühl* as representing undifferentiated or fused organic sensation and inclusive of unitary bodily states (Lust, Unlust and the like), and exclusive of organic sensations in so far as these are localized or analyzed.

[1]E. H. Weber: Tastsinn und Gemeingefühl, in R. Wagner's Handwörterbuch d. Physiologie, 489-91.

[2]See Weber's peculiar distinction between *Empfindung* and *Vorstellung der Empfindung*, *op. cit.*, 485-7, and 489-91, in which he maintains that only that which changes, or, more precisely, which can be made to pass through a minute and varied series of changes through the introduction of movements, is susceptible of separation from the self, of spatial reference and perceptive clearness. The subjective, on the other hand, represents not so much the *self-referred* as that portion of the raw material of sense which because it is relatively constant and unchanging remains unreferred, unlocalized, hence chaotic, vague, unclear. For a somewhat similar emphasis on the correlation of clearness and minute spatial delimitation and reference, see E. Meumann: Zur Frage der Sensibilität der inneren Organe, *Archiv für die gesamte Psychologie*, IX, 1907, 57, 58. Where Weber neglected the possibility of differentiation and analysis through any other means than movement and localization, Meumann overestimates

apperceptive clearness is obviously responsible. At any rate, it is obvious that the difficulty cited applies mainly to our knowledge of the general contours and positions of our internal organs rather than to the observation of the varieties of sensation to which, functioning as wholes, the lungs, heart, diaphragm, stomach or internal circulatory system may give rise.

This experimental difficulty finds a later version in the statement of Külpe[1] that the customary experimental appliances for the facilitation of the isolation of constituents are here useless. This objection likewise rests on a false parallel between external and internal conditions. In the first place there is no reason for assuming that even if appropriate methods were applicable any great differentiation of sensation in different portions of the same organ would be discovered. In the second place, psychology is interested less in the sensory mapping of the viscera than in the peculiarities of organic sensations themselves, and the impossibility of detailed surface exploration by no means precludes the isolation of individual qualities or masses of sensation due to the functioning of an organ or tissue as a whole, through indirect stimulation by drugs, chemicals, or by taking account of various known correlations of external stimuli and internal *Mitempfindungen*.

There are, however, according to many writers, more serious obstacles in the way of the analysis and localization of internal sensation. Certain inherent peculiarities in the normal anatomical and physiological conditions of production of organic sensation manifested on the conscious side in its intrinsic vagueness, diffuseness, feebleness and violence, effectually debar it from the realm of clear perception. Among the unfavorable conditions specifically mentioned we find the scanty sensory innervation of the viscera[2], the central or diffuse and

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the clarifying value of the spatial reference and definition contributed by the visual image. Both positions, in our opinion, exaggerate the importance of one among several possible contributory factors in attentional clearness.

[1] O. Külpe: Outlines of Psychology, 140.

[2] According to Müller, who perhaps set the tradition on this point, the feebleness and vagueness of internal sensation is the necessary correlate of the fact that the interior of the body is innervated chiefly by the sympathetic system, the primary office of which is the mediation of reflexes, not of sensation, and the ganglia of which were in Müller's day believed by some actually to function as the suppressors of sensation. Müller himself seems to have assumed, as Mackenzie, Head and others have more recently maintained, that the sympathetic possesses in its own right little or no entrée to the sensorium, depending, in cases of high intensity, on a forced passage over the fibres of the cerebro-spinal system correlated with pain. See J. Müller: Elements of Physiology (tr. by Baly, 1837), Vol. I, 723, 795-9.

general character of the adequate stimulus, and the possession of a peculiar secondary conduction path functioning in the case of pain.

Among those maintaining the essentially feeble and liminal or subconscious character of the internal sensory processes in their non-painful phases are Lotze,[1] Höffding,[2] Wundt,[3] and Ladd.[4] According to all, the affective tone correlated with the consciousness of bodily state (cœnæsthesia) is far more prominent than the semi-chaotic mass of sensation to which it is attached, and which may even be totally submerged. This state of affairs is indeed susceptible of explanation as a case of affective summation. On the face of it, however, so extreme a statement gives rise to the suspicion that introspection is here adding to the credit of feeling at the expense of sensation,[5] and that the construction of Kröner and Lagerborg, who maintain that in such cases the sensation mass itself constitutes the feeling or affective tone,[6] stands closer to the truth.

Some peculiar mechanism of transmission for higher intensities lies presumably at the root of the sudden jump which Weber, Wundt, Külpe and others assert to take place when organic sensation abruptly rises from this vagueness and indistinctness into pain of such violence and high affective value that accurate observation of it is impossible.[7] The requisite conditions of such a mechanism might indeed be in part fulfilled on the hypothesis of a secondary conduction path offering considerable resistance to its more peripheral synapses, in its more central portions a low limen and considerable capacity for explosive and intensive functioning. Feebleness and violence, at any rate, appear alternately as the basis of explanation of the ease with which organic sensation customarily

[1] R. H. Lotze: Medicinische Psychologie, 281.
[2] H. Höffding: Outlines of Psychology, 1891, 225-6.
[3] W. Wundt: Physiol. Psychologie, 1902, II, 42-4.
[4] G. T. Ladd: Psychology, Descriptive and Explantaory, 1894, 175, 184.
[5] It is noteworthy in this connection that the four authorities quoted are all exceedingly liberal in their estimates of the variety and richness to be ascribed to the affective elements in general. They are also all more or less inclined to identify the distinctive feature of pain not as a sense quality, but as feeling moment superadded to or extinguishing the latter.
[6] E. Kröner: Das körperliche Gefühl, 1887; R. Lagerborg, *op. cit.* See also A. Horwicz: Zur Lehre von den körperlichen Gemeingefühlen, *Vierteljahrsschrift für wiss. Philosophie*, 1880, IV, 273-310, for an excellent discussion of organic sensation as feeling, disposition or mood.
[7] Wundt: *op. cit.*; Külpe: *op. cit.*, 140, 146; Weber: *op. cit.*, 22. According to Weber, this state of affairs is due, at least in part, to the lack of developed *Tastorgane* to protect the terminations of the internal nerves from over-stimulation.

eludes the attention. The possible neurological obstacles in the way of organic sensation as well as the query as to its essentially painful character will come up for discussion later. In the meantime, however, it is worth noting that the authorities quoted cite the high affective value and commotion produced by organic sensation in its supraliminal phases as irremediable obstacles to observation, without apparently pausing to consider whether the gap between liminal and violent organic sensation is actual and absolute, or merely the effect of the normal absence of any incentive to attend to intermediate intensities; all organic sensation possibly appearing liminal in its marginal vagueness until of a strength to secure its automatic admission to the focus of consciousness.

While the trait of unanalyzability has been in part ascribed to the defective and undeveloped functioning or lack of elaboration of the nervous mechanism, that of unlocalizability especially has been referred[1] to the central or general character of the adequate stimuli. If for instance the immediate effect of an alteration in the composition of the blood on the brain centres or on the peripheral nervous system as a whole constitutes the adequate stimulus, the operation of the ordinary mechanism of localization or reference becomes improbable. A word of caution is, however, here in order. While the tonic action in reflex centres of the condition of the blood is an accredited physiological fact, the direct effect on consciousness of such action is problematical. Therefore, even though the appeal to conjectural central processes is, in the absence of reliable introspective data and in the interests of special theories and classifications, admissible, such theorization can hardly be permitted to throttle investigation of the facts. The authority of this position is, moreover, weakened by the lack of any detailed working out of the hypothesis, or attempt at adjustment with current doctrines or facts, as *e. g.*, in Kröner's chemical theory, where, in defiance of the generally accepted doctrine of specialization or adaptation in the afferent nervous system and its central stations, absolute indifference of function is tacitly assumed.

In view of the absence of any definite experimental evidence against the possibility of observation and analysis the more hopeful attitude of certain psychologists is worth noting. Beaunis in especial is firm in the conviction of the possibility of training the attention upon the observation and singling out of internal sensations, while Meumann reduces their customary vageness to indefiniteness of localization and the latter primarily to the absence of correlated visual images,[2] leaving

[1] Wundt: *op. cit.*, p. 43. Kröner: *op. cit.*, 28, 31. [2] *Loc. cit.*

open the possibility of the application of other methods to the breaking up and clarifying of organic sensation.

Against the objection that organic sensations are not reproducible, give rise to no free images,[1] it may be also urged that the question remains open whether this defect is intrinsic to the neural mechanism of internal sensation, or transcendable through experimental methods and training. The whole problem of the possibility or impossibility of the investigation on this or other grounds is, of course, intimately bound up with the question whether visceral afferent impulses in general represent rudimentary, atrophied and vestigial sensation, or sense material the further development of which has been arrested, not inevitably impeded by functional or practical causes. We shall, therefore, in a succeeding section, have recourse to the natural history of organic sensation, in its biological, physiological, and histological phases, as a guide to the probable limits of profitable investigation.

2. *Existence of any organic sensation other than pain.* Common sense sanctions the recognition of a wealth of either actively pleasant or only mildly unpleasant organic experiences, hunger, satisfaction, satiety, and the like, yet clinicians, physiologists, and even a few psychologists, notably Wundt, have stubbornly asserted the essentially painful character of all visceral sensation. When, however, the basis for this assurance is examined, its negative and inferential rather than introspective character is quickly evident. In the case of the clinician an over-emphasis on pain is but the normal outcome of his preoccupation with surgical and pathological, *i. e.*, *painful* cases. This prepossession is strengthened by the insensitivity manifested by the viscera under surgical manipulation; the customary stimuli (mechanical, thermal, and chemical) of external cutaneous sensations (pressure and temperature) are here inoperative. Arguing by analogy with the skin, he therefore quickly arrives at belief in the non-existence of any painless phase of organic sensation. We need only pause here to point out the logical conclusion of argument upon these premisses, *i. e.*, the negation of all visceral sensation, painless and painful alike. The Swedish surgeon Lennander has indeed recognized the logic of this position, and achieved the *tour de force* of transferring all the responsibility for the so-called

[1] *Cf.* E. B. Titchener: Organic Images, *Journal Phil., Psych. and Sci. Meth.*, 1904, I, 36 ff. The objection is here raised as a protest against dogmatic assertions that any sensation, just because it is sensation, must be reproducible in image. The author finds evidence that at any rate in minds of certain types, certain organic sensations are not thus reproducible, and enters a plea for further systematic work on the subject.

"visceral" distress of disease and inflammation to the body wall, a doctrine which we shall revert to later. Others, while less extreme, are nevertheless sensible that on the line of reasoning above adopted the emergence of any organic sensation whatever, even pain, is an anomaly to wrestle with. Given the law of parsimony and the fact that the afferent impulses of the viscera do not normally reach consciousness, what explanation is possible of the neural mechanism by which in cases of exaggerated intensity or disease these processes in the guise of pain assail the attention? Would cortical connections functioning perhaps once, perhaps never in the lifetime of the individual persist in the race?

The answer to this problem furnished by Foster[1] is physiologically simple enough, though involving a dubious recourse to the subconscious, and will be touched on later. According to the more radical, however, the pain of disease represents in all probability a later development in the history of the race for the protection of the regions supplied by the older subconscious sympathetic system,[2] and is mediated by some makeshift of a mechanism, such as leakage from the sympathetic or vagus to the pain centres of the external system (Ross, Mackenzie, and Head), or the stimulation through excessive visceral activity or inflammation of nerve trunks of the external system which happen to pass near the organs or tissues affected (Lennander).[3] The paradox of the apparent painful response of the viscera to external pressure in case of disease or inflammation is, moreover, generally reduced to a mere matter of the hyperalgesia of the body wall or skin (Mackenzie and Head).

With regard to the insistence of the surgeon on the non-existence of painless visceral sensation, it will be sufficient here to indicate not only the fallibility of observations conducted under such unfavorable conditions, but further, the inconclusiveness of argumentation on the basis of a doubtful analogy between skin and viscera. The fallacy involved becomes the more apparent when a similar line of reasoning is applied to the external muscle system. The insensitivity of muscle, tendon, and joint, many times demonstrated, probably long retarded the recognition of the 'muscle sense'. Yet to-

[1] M. Foster: Physiology, IV, 284, according to whom pain is mediated by the ordinary afferent mechanism, and represents merely an exaggeration of afferent impulses which are continuously present but unnoticed. Another solution is presented by L. Oppenheimer who has pressed the vaso-motor nerves into service in explanation of internal as of external pain; see his Physiologie des Gefühls, 1899.

[2] J. Mackenzie: Pain, *Brain*, 1902, XXV, 384.

[3] For further discussion and references, see below, Section B.

day the absurdity of a denial of the kinæsthetic sensations bound up with the normal functioning of those tissues would hardly be tolerated. There is every reason for assuming that the adequate stimuli of internal tissues are radically different from those of the external (the cutaneous in particular), being in all probability predominantly chemical in character, as the functions of the internal organs would imply. The doctrine of the insensitivity of the viscera, while in its limited clinical application a fact of prime importance, need therefore in cases of normal functioning carry little weight for the psychologist prior to more exact and extended observations.

Among the physiologists[1] the parallel doctrine of the essentially abnormal and painful character of any sensation in the viscera represents a tradition handed down from Weber and based primarily upon a somewhat hasty inference from the absence of protective 'touch' organs similar to those of the skin on the mucosa or endodermal surfaces and of the lack of temperature or pressure sensations in the more peripheral portions of the alimentary canal,[2] secondarily perhaps on the physiologist's tendency toward preoccupation with the purely vegetative and nonconscious functions of the visceral nervous system. In view of the counter evidence available on the introspective side in the writings of J. Müller, Bain, Sherrington, Helmholtz, Lehmann, Ebbinghaus, James, Beaunis, Titchener, and Meumann, we need not at this point enter into further discussion on the question.

3. *Plurality of Qualities.* For those who grant the possibility of painless visceral sensation, the terminological question of the variety of sense qualities, the number of tones, so to speak, in the organic gamut, next arises. In view of the general conviction that analytical methods are unavailing in this sphere, the distance covered by individual observers is worth

[1] *Cf.* Gruenhagen's Lehrbuch der Physiologie; W. Nagel's Handbuch der Physiologie des Menschen: M. Foster's Physiology; and W. H. Howell's Text-book of Physiology, 1906, all of which virtually ignore any quality other than pain. This is frequently, however, little more than an assertion of the essential difference between internal and external surfaces, and the lack of response of the former to environmental stimuli. When the physiologist happens to think of a true functional visceral complex, as hunger or nausea, he is usually moved to make an exception to his 'no non-painful visceral sensation' rule. Further, his concept of pain is largely physiological; pain is simply the harmful to the organism. Inasmuch as he is usually uncertain, on the psychological side, whether the painfulness of pain is a sensory or affective attribute, and intrinsic or adventitious, his applications of the term may frequently be misleading: his physiological pain may connote, on the psychological side, a quite indifferent sensation.

[2] *Op. cit.*, 562, 489, 497. See also Meumann: *op. cit.*, 43-47.

noting. A brief survey of the literature reveals the case as follows. In spite of the alleged inaccessibility of the organic factors to detailed examination, the general trend of opinion guarantees their uniqueness and variety—uniqueness as over against the qualities of the muscle-skin continuum, variety as among themselves, in virtue of their origin in different organs and tissues. The significance of this apparent liberality must, however, not be overrated; the above statement must be hedged in with certain reservations. In the first place, it must be admitted that, in individual cases, notably in the psychologies of Ladd and Wundt,[1] this class of sensations fares badly: the richness and variety of coloring ascribed to organic content are invariably inversely proportionate to the importance and variety attributed to the affective elements proper. In the second place, the significance of the unanimity above cited is greatly lessened when the varying rigidity with which introspective criteria and the concept of quality are applied by the different writers is taken into account. The varying importance and significance attached to the doctrine of specific energies here plays an important rôle. Laxity in regard to this principle is usually manifested in a tendency to accept offhand any superficial difference, topographical or practical in origin, as qualitatively valid. In general, exact introspective evaluation and analysis is absent and classifications are based upon purely extrinsic data. Müller, Bain, Sherrington, Helmholtz, Ebbinghaus, James, Beaunis, Titchener, and Meumann admit plurality of organic qualities in some form or other, but with what discrepancy in the details! Müller,[2] *e. g.*, while conceding a variety of shading in visceral sensation, nevertheless unhesitatingly ranks it in the same sub-class with the external cutaneous and muscle sense elements, pressure, temperature and pain, themselves susceptible of many qualitative fluctuations and gradations. The remainder of the authors listed hold apparently to the uniqueness of the organic contribution as over against the muscle-skin continuum, but this can only be predicated with certainty of Titchener, Ebbinghaus and Meumann. In general, the 'divisibility of organic experience into two great categories, the pleasurable and painful, is admitted, but in the detail of further classification great diversity exists.

The prodigality with which Bain[3] lavishes his sensory rubrics is especially conspicuous. Various forms of muscular, nervous, circulatory, respiratory, and digestive 'feelings' are

[1] G. T. Ladd: *op. cit.*, 168; Wundt: *loc. cit.*
[2] Müller: *op. cit.*, 1324 and 1087.
[3] A. Bain: Senses and Intellect, 1864, 118ff.

cited, and the peculiar attributes of fatigue, exhilaration, depression, suffocation, hunger, satiety, nausea, and various other bodily comforts and discomforts described in general terms. Comparison or detailed analysis is, however, not attempted, with the result that ramifications overlap and the classification remains of dubious value except as a testimonial of the author's absorbing interest in the subject.

Beaunis'[1] voluminous schema is likewise unreliable, based as it evidently is on a half dozen different principles of classification. Differences in the way in which sensations come to consciousness, variations in their mode of origin or occasion of appearance (*i. e.*, in the normal exercise of function, in premonition of organic needs, or in surgical manipulation), their pleasant or painful character, rather than observed qualitative distinctions form the *raison d'être* of his eightfold division. The casual character of the classification stands revealed in the headings, 'organic sensibility', 'organic needs or cravings (*besoins*)', 'functional sensations', 'cœnæsthesia', 'emotional sensations', 'specific sensations', 'pain', and 'pleasure', which obviously indicate groupings or points of view significant to the physician or biologist, but hardly to the psychologist. While Beaunis is usually quoted as having testified to the plurality of organic qualities, he really never comes to close quarters with the question, but concerns himself, as a matter of fact, almost exclusively, with the determination of the number of the internal tissues sensitive, and the extent of the rôle played by organic, more especially, *muscle* sensation, either in its conscious or subconscious phases, in all adjustments of the organism.

Horwicz seems to occupy a position midway between those of Bain and Sherrington. While apparently admitting specific sense qualities, he maintains that many of them are obscure and indescribable.[2]

Sherrington,[3] though primarily interested in the problem of pain, admits the existence of many organic sensations both pleasurable and painful which are far from obscure. Both functional activity and the call for repetition of this activity are plainly conscious, although they must remain forever indescribable in virtue of the fact that the physical offers no adequate and unequivocal ultra-individual symbols for these

[1] H. Beaunis: Les sensations internes, 1889, 3.

[2] A. Horwicz, *op. cit.* See also J. Sully's review, *Mind*, 1882, VII, 302-3.

[3] C. S. Sherrington: in E. A. Schäfer's Text-book of Physiology, II, 971-3. There is also in "The Integrative Action of the Nervous System," 1906, 130, an implication that the 'proprio-ceptive' qualities of the deeper tissues are specific.

sensations. The variety of organic sense qualities is, however, in this discussion an implication merely. In view of the tendency occasionally appearing among physiologists to identify pleasantness and unpleasantness with pure sensory qualities (*i. e.*, the physical sensations of pleasure and pain), it is interesting to note that Sherrington, along with the three preceding writers quoted,[1] may perhaps best be described as regarding organic sensations as modes or shades of the two basic qualities of bodily comfort and discomfort.

For Ebbinghaus,[2] the sensory contribution from the internal tissues is as varied and extensive as for Bain. Hunger, thirst, satiety, discomfort, nausea, fatigue, energy, oppression, suffocation, buoyancy and the like are listed as arising from the digestive, muscle, and respiratory systems respectively, along with certain more diffuse organic states, such as excitement, unrest, relaxation, depression, dullness and drowsiness, the probable origin of which is assigned to changes in the circulatory system. The question of the possible complexity, similarity or relationships of these states is not raised. The implication is that each represents a specific sensation peculiar to the system from which it arises, each organ possessing '*eigenartige Empfindungen*'. Further, sensitivity to pain is declared to be lacking only in connection with the substance of the brain, and the mucosa of the intestines and stomach.

Titchener (see J. M. Baldwin's Dictionary of Philosophy and Psychology, II, 218) distinguishes, apart from skin and kinæsthetic sensations, a number of organic qualities for which the alimentary canal, circulatory, and respiratory systems are held responsible.[3]

Meumann,[4] in a recent article, has expressed himself in favor of a great multiplicity of organic sensations (*innere Tastempfindungen*), on the basis not only of the *a priori* argument of origin in different organs and tissues—unstriped muscle, serosa, glands, capillaries, the alveoli of the lungs, etc., and of usefulness to the organism, but also as the result of his own carefully recorded introspections and of certain observations of d'Allonnes. This qualitative diversity is obscured, he maintains, only by the indefiniteness of localization of the

[1] Weber and Kröner might be included in this list.

[2] H. Ebbinghaus: Grundzüge der Psychologie, 1902, I, 426-32. The 'Abriss der Psychologie', 1908, p. 49, contains perhaps the most positive statement in the whole literature. "Die durch sie vermittelten Empfindungen stehen ebenso selbständig und eigenartig nebeneinander und neben den übrigen Empfindungen wie Farben neben Tönen und Geschmäcken."

[3] See the same author's Primer of Psychology, § 21; Text-book of Psychology, §§ 56 ff.

[4] *Op. cit.*, 56-8.

sensations, and their deficiency in correlated visual images by which qualitative isolation might be facilitated.

James, while assigning tremendous importance to organic sensation and expressly stating that at least for pain the 'internal organs have their explicit qualia of sensation just as different areas of the skin have their respective local signs',[1] contents himself characteristically with the assertion that 'our whole cubic capacity is sensibly alive',[2] and with casual references to 'precordial anxiety', the 'euphoria arising from the respiratory organs', to the 'pang in the breast', a 'fullness in the breathing', etc., without at all committing himself to the composite or elementary character and specific differences of these bodily symptoms. It is further to be noted that a large proportion of the physiological 'reverberations' reported by him as constituting emotion are obviously cutaneous and muscular rather than visceral.

Finally, there is a small group of writers, who, while attributing a certain diversity to organic sensation (*i. e.*, not limiting it strictly to the category of pain), recognize its close kinship if not identity with the external (cutaneous and muscular) continuum. In this class belong Müller, Külpe, Goldscheider, and Calkins,[3] all of whom are inclined to find in the quality of pressure a sufficient explanation for all visceral complexes. A similar tendency to posit a single muscle quality common to both skeletal and visceral tissues may also be noted. Weber, Bain, and Beaunis may here be mentioned, although the most explicit statement occurs perhaps in Lagerborg.[4] Lagerborg himself, however, is unwilling to commit himself unconditionally to the identity of this quality in the external and internal tissues, owing largely to the differences in the minute structure of the latter. As a matter of fact this phase of the question seems to have rested so far on anatomical rather than psychological observations and comparisons.

In conclusion, it must be admitted that there is nowhere in the literature any consistent attempt to indicate similarities

[1] W. James: Principles of Psychology, II, 156.

[2] *Op. cit.*, 451.

[3] Müller: *loc. cit.*; Külpe: *loc. cit.*; Goldscheider, Gesammelte Abhandlungen, I, 1898, 47-8; M. W. Calkins: Introduction to Psychology, 1901, 84-6. Mention may here also be made of E. Becher who has recently on the basis of certain experimental observations on the stomach and œsophagus (see *Zeitschrift f. Psych. und Phys. d. Sinnesorg.*, 1908, XLIX, 341 ff.) arrived at the conclusion that feeble sensations of warmth, cold and pressure may be obtained from the œsophagus, and that all the other viscera are probably insensitive. The sensations usually ascribed to the viscera are either centrally excited or arise from the peritoneum, pleuræ, diaphragm, or body wall (hence are presumably closely related to the external continuum).

[4] *Op. cit.*, 16 and 17.

and relationships in the states under discussion, nor is there even any substantial agreement in the use of single terms. Hunger, *e. g.*, is identified by Kröner with a diffuse, unlocalizable feeling (preferably, faintness), by Sherrington and others with a localized sensation mass in the epigastrium. Neither, moreover, is there any light shed upon the subject of specific liver, kidney, lung or heart sensations in the discussion of psychopathic or emotional cases by Störring, Kraepelin, Janet or Lange, as we had hoped. It is plain that the solution of the problem of a scientific terminology and descriptive basis for organic sensations must be preceded by a more minute examination than has yet been attempted (in any of the authors quoted) of the sense material concerned; and especially by a detailed comparison of visceral with external (kinæsthetic and cutaneous) sense qualities. Such a procedure should, at the very least, establish some large likenesses or unlikenesses (other than pleasantness or unpleasantness) which may serve as the basis of a stable classification.

Summary. While, on the testimony above cited, the existence of a large body of sensations not usually externally referred is incontestable, the precise significance of this fact remains to be determined. The ground of distinction between organic and special sense experience stands greatly in need of definition.

The fusability, absence of memory images, unanalyzability, lack of cohesiveness with other sensations, unlocalizability, capacity for eluding the attention, and other features ascribed guardedly or confidently in various quarters to our organic experience, demand critical verification. Finally, whatever may be the actual status of these sensations, the investigation of what represents apparently the least developed and systematized sphere of our consciousness may reasonably be expected to throw a new light upon the processes of localization and attention. Even though the results of this study should fail to assist in the solution of any psychological problem, a systematic account of these processes and their localizability in the normal subject may be not without value to the pathologist and clinician.

Histological, Pathological and Physiological Evidence

An investigation of the psychology of internally initiated sensation can hardly be undertaken without first attempting to stake out the possibilities in keeping with neurological conditions. As already indicated, such problems as the differentiation of qualities and the range of sensitivity in the internal tissues, of the possibility of localization and analysis,

and the significance of the former, are clearly related to the histological problems of the variety and distribution of afferent nerve endings and the possibilities of diffusion inherent in the peripheral or central neural mechanism. A brief survey of the literature of the sympathetic system and the sensory innervation of the internal tissues has accordingly been undertaken and the consensus of expert opinion upon the number of tissues sensitive, kinds of endings, relative fewness or multiplicity of nerve fibres, conduction paths, brain centres, and the like collated. For information upon the immediate and remoter origin of internal sensation, according as determined directly by chemical or mechanical changes in the circulatory or muscular tissue, or indirectly through the medium of reflexes or irradiation in the central system, recourse has been had to physiology.

The result has proved somewhat disappointing. Authenticated knowledge of the sensory endings in any given tissue and their conduction paths from periphery to cortex is meagre. Direct evidence based on the results of degeneration or experimental stimulation of the nerve elements figures to a surprisingly small extent and is eked out largely by inference from more or less ambiguous introspective data. *E. g.*, the existence even of afferent endings in plain muscle tissue is argued from the fact of locally referred pain in renary and biliary colic. The insecurity of any conclusion based on such premisses becomes apparent in view of the doubt cast upon the trustworthiness of localization by certain clinicians and pathologists, *viz.*, Mackenzie and Head, in their doctrine of referred pain. The need not only of a more thorough-going exploration of the centripetal paths through the posterior root ganglia and cord but also of a more cautious use of the facts of apparent localization, will become apparent as we proceed. On the whole we can extract from histology in its present status very little evidence bearing directly on organic sensation. So far as sensory in distinction from afferent innervation of the tissues is concerned, psychology is largely thrown back upon her own resources.

With regard to the evidence of pathology, the question raised by Mackenzie whether internally initiated sensations have central pathways and end-stations in their own right, or are dependent for access to the higher (*i. e.* conscious) centres upon pathways borrowed from the sensory system of the body wall, is an important one. Its bearing upon the existence of unique and specific sensory qualities in the viscera is, in view of the current assumption of the all-importance of the end-station and the indifference of the process in the nerve fibre, obvious, and will be examined in some detail.

A. Anatomical and Histological

1. *End-organs in internal tissues.* Only two kinds of receptive tissue[1] have so far been described in the internal organs, the free sensory endings and the Vater-Pacini corpuscles. Of these, the latter (which occur also in the dermis of the hand and foot, near the joints, in the periosteum of certain bones, in tendons, intermuscular septa, and even in muscle) are found in the epineural sheaths of certain nerve trunks and near large vessels, and are numerous in the peritoneum, mesentery, pleura and pericardium.[2] The free sensory endings, which are usually branching and nodulose, are found in epithelial connective and plain muscle tissues, and are distinguished from motor endings mainly by terminating on, rather than in, the muscle fibre.[3]

The bearing of this limited differentiation of sensory endings on the question of the number of specific differences in organic sensation is of course debatable. If we accept the theory of specific energies, which maintains that the nerve process excited in any peripheral fibre by a physical stimulus is *per se* indifferent, *i. e.*, of one kind only, and that even if the nerve fibre itself is excitable by a wide range of stimuli known as common stimuli (thermal chemical, mechanical, electrical, etc.), mere difference in character of the stimuli is not sufficient to secure differentiation of sensory response, but that the latter presupposes an elaborate neural evolution in which appropriate end-organs to select one form of stimulus, and protect the nerve ending from all other forms are developed along with a central mechanism for securing differentiation in consciousness of the corresponding sensation,—if we accept this hypothesis in all its strictness, the implication for organic sensation is of course pretty straightforward: evidence for a plurality of organic sense qualities is lacking. Two objections to such a conclusion may, however, be raised. First, it is not improbable that further histological research may discover slight differentiations in the chemical composition if not in the structure of the

[1] A possible third is represented by the occurrence in the mucosa of the ileum of fibrils terminating on or near nerve cells similar to tactile corpuscles. Poirier et Charpy: Traité d'anatomie humaine, 1901, IV, 92.

[2] A. A. Böhm, M. von Davidoff, and G. C. Huber: Textbook of Histology, 1904, 174. It is unfortunately not always clear whether human or mammalian tissues in general form the basis of such deductions.

[3] There is, however, a difference of opinion here. G. C. Huber maintains that the motor fibre itself ends *on*, not in, the muscle fibre, leaving the sensory-motor distinction the more in doubt. *Cf.* The Sympathetic Nervous System, *Journal of Comparative Neurology*, 1897, VII, 192.

tissues in which the nerve fibres end or in the terminal nodules themselves, such as to render them specifically adapted to the chemical reactions or substances peculiar or significant to the tissues in which they occur. The fact that a relatively small variety of end-organs has been discovered in the mucous membrane of the mouth which nevertheless possesses a sensitivity to temperature, pressure, and pain (not to mention taste) as acute or more so than that of the external skin, lends color to this hypothesis. Secondly, the tendency is now showing itself to abandon the doctrine of the identity of the nerve process in all fibres and the more rigid conception of specific energies, and to suggest, what indeed was the older view, that the range of excitability of a single nerve fibre may include stimuli slightly different in quality, each of which is capable of giving rise to its own peculiar nerve impulse, which when transmitted to the centre may be sensed as an individualized sensation quality.[1] Apply this doctrine to the free sensory endings of the viscera, and the impossibility of an extended gamut of related organic qualities vanishes.[2] In the absence of any deterring histological or physiological evidence, we may at least admit that the field remains open to psychological exploration and experiment.

2. *Distribution of afferent innervation.* The occurrence of 'sensory' nerve fibrils in the epithelium of the bladder, œsophagus, tongue, lung, liver, pancreas, stomach, intestine and the like; in the connective tissue of the heart, lung, and eyes, of the dura mater, and of certain mucosa; and probably in the plain muscle tissue of the hollow viscera (heart, intestine, ducts, glands, etc.) is maintained by Barker[3], and more or less substantially sustained by Böhm, by the anatomists Poirier and Charpy, and by various others with certain reservations. It is to be noted, however, that the term 'sensory' is to a certain extent used interchangeably with 'afferent', and that the classification of a nerve as sensory is in many cases based merely on the supposedly non-motor character of the endings, unsupplemented either by the tracing of the fibre back to the posterior root ganglion, or the testing of the stump for pain. Further caution is suggested by the possibility that many of the fibrils listed, even if not motor or secretory, represent nerves concerned only with the mediation of reflexes in the cord or lower centres, and incapable of projecting a sensory

[1] W. Nagel: *op. cit.*, 13, 15.

[2] As a third possibility we might suggest the structural degeneration of the end-organ, after a functional and central differentiation had become established.

[3] L. F. Barker: The Nervous System and Its Constituent Neurones.

impulse to consciousness.[1] The evidence and line of argument by which the neurologist or physiologist arrives at his conclusions must therefore be scrutinized in detail.

The general method of procedure by which the afferent or sensory innervation of the various tissues is established is as follows. Gross anatomical and experimental methods come first in point of time. The stumps of the general nerve supply of any region were tested by the early anatomists, either for outward evidences of pain, or for simple reflex discharge in related motor channels, and inferences on the sensitivity or insensitivity of the tissues innervated made accordingly. Thus the insensitivity to pain of the sympathetic nerve as tested in the experiments of Majendie was assumed to establish the insensitivity of the innervated tissues, notably the intestine and its outlying glandular systems,[2] while the sensitivity of the stump of the vagus was, on the contrary, assumed to indicate the sensitivity of the region of its peripheral distribution, inclusive of heart, lungs, and stomach in particular. The ambiguity of tests of this type is readily apparent. The test based upon the stimulation of the stump for pain symptoms is valid only for pain nerves and overlooks the possibility of the presence of non-painful sensory fibres, at the same time that it ignores the probability that the adequate stimulation of visceral fibres is serial, intermittent, and summating. Even the success of the test in eliciting evidences of reflex action indicates at best the existence of a reflex arc through the cord or lower centres.

As a matter of fact, later application of these very tests has resulted in the reversal of Magendie's findings. Evidences of pain following stimulation of the sympathetic (through the cœliac or solar ganglia, or the splanchnic nerve) have been demonstrated by Flourens, Brachelt, Mayer, and Müller.[3] Indeed, to-day the responsibility for pain in organs below the diaphragm (the stomach, liver, and pancreas in particular) is usually shifted to fibres reaching these organs via the sympathetic rather than the vagus,[4] although the innervation by the two systems is to a large extent overlapping. In general, however, with refinement of methods and a growing discrimi-

[1] The possibility that even purely afferent or reflex fibres may under conditions of excessive stress borrow from other systems a path to the higher centres will be examined later. The *normal* sensory functioning of the fibres is alone here in question.

[2] The innervation of the intestine by both sympathetic and vagus (*cf.* Langley) was evidently not then recognized.

[3] Müller: *op. cit.*, 646, 705, 711 ff. These tests were usually, of course, performed upon animals.

[4] *Cf.* J. N. Langley: The Autonomic Nervous System, *Brain*, 1903, XXVI.

nation in the use of the terms affective and sensory, the tendency has sprung up among neurologists to distrust the sensory function of any fibre travelling with the sympathetic unless its passage direct from the posterior root ganglion *through* the sympathetic ganglia without termination can be demonstrated or inferred. That is, the only clear title of any fibre to sensation lies, histologically speaking, in its claim to enrollment as a cerebro-spinal fibre. On this basis the bias for the sensory innervation of the tissues supplied by a cranial nerve such as the vagus (the thoracic viscera and stomach especially), or by a spinal nerve, as the phrenic (arising from the third to fifth cervical roots and supplying the pleuræ, pericardium, and diaphragm),[1] is strong. Further, the demonstration of individual cerebro-spinal fibres (*i. e.*, of fibres passing without interruption from the posterior root ganglion cells to peripheral terminations in visceral tissue) in the sympathetic plexuses,[2] and the observation that certain histological peculiarities characterize such fibres, have suggested a practical basis for the distinction of afferent from efferent fibres (more particularly of sensory from

[1] K. Hasse: Handatlas d. sensiblen und motorischen Gebiete der Hirn- und Rückenmarksnerven.

[2] The fact that practically no afferent fibres occur in the gray rami by which efferent fibres of sympathetic origin return to the spinal system for distribution to the body wall, which must therefore draw its afferent or sensory supply directly from spinal ganglia (P. J. Cunningham, Textbook of Anatomy, 704), has led to the hypothesis that a large number of the afferent fibres of the viscera, in homology with those of the body wall, do not reproduce the course of the efferent autonomic system and terminate in sympathetic ganglia, to resume their course in the form of dentritic or axone fibres from sympathetic cells, but behave in every way like ordinary sensory nerves. Kölliker (*cf.* Quain, Elements of Anatomy; 350,), Gaskell (*Journal of Physiology*, 1886), Huber (*Journal of Comparative Neurology*, 1897, VII, 131), and Langley (Schäfer, *op. cit.*, 687), all recognize the presence in the sympathetic strands of a number of these (strictly speaking) *non*-sympathetic or *cerebro-spinal* fibres, having their trophic centres in spinal ganglia.

From this point of view the sympathetic ganglia would, of course, possess no automatic or reflex functions but represent merely a provision for the wider distribution of the sphere of influence of all preganglionic efferent elements, most of which end in relation with several different ganglion cells. To be sure, certain histologists, notably Dogiel, maintain the presence in the sympathetic ganglia of sensory or afferent cells, the axones of which end in association with efferent cells in the sympathetic system or cord to form reflex arcs. Schulze, moreover, finds among smooth muscle fibres sensory ganglion cells with one long central process, which, he suggests, is ordinarily concerned with the mediation of reflexes (presumably in sympathetic ganglia), but in cases of muscular spasm may give rise to pain. Lenhossek is also of the opinion that the sympathetic ganglia represent either relay or terminal stations for certain of the sensory fibres (Huber, *op. cit.*). The evidence on these points is, however, insufficient, and even if such a condition of things exists it obviously holds good only for a small proportion of the afferent fibres.

merely afferent), and for a more accurate discrimination of sensitive from non-sensitive tissues.

The marks by which the character of any nerve as cerebrospinal and afferent, hence presumably sensory in function, are established, are three. First, the degeneration test, by which continuity of the fibre in its complete course from posterior ganglion to endings in any tissue is traced. Of evidence collected in this fashion there is only too little. Such as does exist points to the sensory innervation of the mesentery and the peritoneum (where the fibres end in Pacinian bodies), and of the bladder (in the frog).[1] Secondly, medullation of the fibres,[2] and thirdly, their large size or diameter. Neither of these last criteria is held to be absolutely valid in itself. They are, however, accepted as indicating the sensory character of the nerve endings in blood-vessels, heart, dura and pia matter,[3] bladder, gall-bladder and mucosa of the intestines.[4]

With regard to the more minute histological indications of differentiation, the presence of free nerve fibrils in non-motor or non-secretory tissue (connective or epithelial) is assumed to represent afferent or sensory innervation. This applies to the vascular tissues, to the bladder, intestines, and endodermal or epithelial tissues in general.[5] On the other hand, such free fibrils as occur in the walls of hollow muscular organs—the alimentary canal, biliary and renal ducts, blood vessels, heart, etc.—are distinguishable from motor endings by minor characteristics only,[6] and their sensory function is largely inferred from the apparent sensitivity of these tissues in colic, renal and biliary inflammation, angina pectoris, migraine, etc.[7]

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[1] Huber: *loc. cit.*

[2] It is a generally accepted fact that only post-ganglionic fibres—*i. e.* the fibres taking origin in ganglionic cells and representing the post-ganglionic continuation of the paths of fibres which have terminated around these cells—are unmedullated. Since this is known to be the rule with efferent sympathetic nerves and since many medullated fibres have been traced back to the dorsal ganglia, the *a priori* assumption is usually made that a medullated fibre in the peripheral twigs of the sympathetic represents an afferent or sensory neurone of the spinal system.

[3] Böhm, Davidoff and Huber: *op. cit.* Sensory endings in the fibro-elastic tissue of many vessels, their endings beset with varicosities, have been recently demonstrated by Dogiel, Schemetkin, and Huber (p. 223). For endings in the inner lining of the larger vessels and in the endo and pericardium, see p. 215; in the dura mater, p. 437.

[4] Stöhr: Lehrbuch d. Histologie.

[5] Van Gehuchten: Anatomie du système nerveux, 1901, II, 531. See also Stöhr: *op. cit.*

[6] *Cf.* Schultz: Die glatte Musculatur der Wirbelthiere, *Archiv. f. Anat. u. Physiol.*, 1895.

[7] Barker: *op. cit.* See also Huber (*op. cit.*) for the suggestion that this line of reasoning is misleading, especially in view of the fact that sensory fibres have been traced into the *epithelium* of certain of the hollow organs.

The first line of argument overlooks the possibility that the fibres may be trophic in function, the second, the probability that other (sensitive) tissues than those immediately concerned in contraction or spasm may be involved. In itself, it is obvious, either line of reasoning goes only a short way unless supported by the system of experimentation and inference outlined above.

In summary, it must be granted that there is hardly a tissue in the interior of the body (mucosa, serosa, or plain muscle fibre) which may not be regarded as possessing an afferent, and thus potentially, a sensory neural equipment. Further, a number of minor lines of evidence converge to indicate the sensory function of this equipment in a large number of these tissues. That for the plain muscle fibres is perhaps the weakest.

According to the calculations of Langley, the proportion of afferent to efferent fibres in the sympathetic system in general is about one to ten. While this seems to corroborate Weber's reference of the feebleness of internal sensation to the fewness of the innervating fibres,[1] it is perhaps more accurately represented as a device for the intensification of the internal sensory impulses through summation. The branches of a single fibre are distributed over a wide area, and the neural effect due to the stimulation of a large number of terminal fibres is thus presumably concentrated in a single centripetal impulse. Further, a large number of the viscera (the thoracic organs, stomach, liver, intestines, and probably liver and pancreas in particular) receive a double afferent innervation, *i. e.*, from both vagus and sympathetic.[2]

3. *Pathways of visceral impulses in the spinal cord.* While the peripheral equipment of the internal tissues appears thus to extend almost indefinitely the possibility of sensation, a further question arises as to the provision for the transmission in isolation and in their full strength, of the sensory impulses to the cortex. First, does the afferent neurone possess a clear pathway in its own right to the cortex (and hence to consciousness) as does for example the sensory neurone from the exterior; or does its axone terminate in association with motor cells in the cord or lower centres, if not actually in a sympathetic ganglion, thereby indicating the function of the neurone to be merely reflex?[3] Secondly, even if a clear centripetal

[1] *Op. cit.*, 491.

[2] *Op. cit.*, 29.

[3] As would apparently be demanded by J. Müller's conception of the afferent sympathetic. See *op. cit.*, I, p. 723 f., where he suggests that visceral impulses are unconscious not because suppressed by sympathetic ganglia (Reil's suggestion), but because they function in the mediation of reflexes, and are accustomed to 'sich ausgleichen' in the cord.

pathway may be assumed, are there no peculiarities intrinsic to the afferent sympathetic neurone (such, *e. g.*, as the absence of medullation, the multipolar character of the cell bodies and the complexity of their terminal fibre connections) which make for diffusion, poor localization, or feeble intensity?

With regard to the first question, the fact that the afferent 'sympathetic' supply to the unstriated muscle fibres, glands, and blood vessels of the skin and body wall demonstrably does not even pass through a sympathetic ganglion but emerges directly from the posterior ganglion to join the postganglionic efferent fibres supplying the same general region, lends color to the probability that the homologous system of internal afferent fibres is quite like ordinary cerebro-spinal nerves, and that an unbroken and medullated course from the posterior ganglion to their final peripheral division into terminal fibrils may be predicted for them. As a matter of fact, so far as an actual examination of the state of affairs has been made, by means of the degeneration method or the tracing of medullated fibres to the very periphery, the evidence goes to show that the majority of the afferent fibres of the white rami communicantes (the visceral branches of the spinal nerves), and their peripheral continuations have their trophic centres in the posterior root ganglia just as do the cutaneous sensory fibres, and enter into no actual connections with sympathetic ganglion cells.[1] Further, experimentation has thus far failed definitely to demonstrate reflex action in any sympathetic ganglion,[2] hence the probability that the afferent fibres neither end themselves in sympathetic ganglia, nor give off any branches for that purpose, but afford a direct pathway to the cord with no opportunity for the shunting off of the sensory impulse into peripheral reflex paths, is materially strengthened.

The question whether the route through the posterior sensory ganglia is uninvolved and in all ways similar to that of an ordinary sensory neurone, *i. e.*, interrupted by one cell body only, is answered in the affirmative by Langley, on the basis both of histological demonstration and the fact that the number of fibres on either side of the ganglia is the same. The feebleness of sensations from the interior is due probably to high liminal values and fewness of nerves rather than to friction in peripheral paths.[3] Barker, however, thinks it probable that the sensory impulses collected by means of sym-

[1] Langley: *loc. cit.;* Cunningham: *op. cit.*, 704; Kölliker in Van Gehuchten: *loc. cit.;* Huber: *op. cit.*, 131.

[2] Exception must possibly be made here of such peculiar peripheral structures as the plexuses of Meissner and Auerbach in the alimentary canal.

[3] Brain, 1903, XXVI.

pathetic neurones pass through a chain or series of individual neurones before arriving at a primary end-station in the cerebro-spinal system.[1] Their fate after joining the dorsal roots of the spinal nerves he admits is unknown, but suggests that some of the sympathetic axones end within the spinal ganglia around the bodies of spinal ganglion cells, and that others pass directly through into the cord. As a matter of fact, Cajal has reported the ending of sympathetic fibres in branches about spinal ganglion cells, and Dogiel asserts that certain sympathetic fibres (which he assumes arise from sensory sympathetic ganglion cells) end about posterior root cells of a new type—small, and with a branched axone, the branches ending in association with a number of ordinary unipolar cells.[2] The evidence on these points is, however, casual and unsupported: Langley's position represents the larger body of facts.

With regard to the further pathway of the afferent impulse through the cord, Langley suggests two possibilities.[3] A certain number of the visceral afferent fibres in all probability serve only to mediate reflexes to autonomous tissues, and are incapable of giving rise directly to sensation,[4] while all the rest are sensory. The distinguishing histological characteristics of the two are yet to be determined, but presumably the upward paths in the cord are poorly developed in the case of the former. It is elsewhere suggested by Langley[5] that the large medullated fibres ending in serous membranes are probably the ones which give rise to (direct) local pain; while the small fibres (ending in connection with unstriated muscle and glandular tissue) give rise to referred pain, that is, acquire an upward pathway and a sensory function only under very unusual conditions.

With regard to the provision in the gray matter of the cord for the transfer of irradiation from visceral to cutaneous fibres (in case of referred pain), there is at present no satisfactory evidence, as Langley admits. Even the irradiation from fibre to fibre of the sympathetic to which Müller[6] had recourse in

[1] *Op. cit.*, 553.

[2] *Cf.* Van Gehuchten: *op. cit.*; also Schäfer: *op. cit.*, 686.

[3] *Op. cit.* M. Meyer's suggestion for *pain* paths of the external and internal systems, *i. e.*, their convergence into fewer and fewer neurones as they pass up the cord, thus eliminating the possibility of exact localization of sensation, seems to find no direct support in the histological literature. See Der Schmerz, 1906, 74.

[4] Mackenzie's hypothesis (see above) would seem to demand the ending of *all* visceral afferent axones in the lower motor centres, and the total absence of centripetal pathways past these centres to the cortex, sensation being effected only through the leakage in the cord to sensory neurones of the *somatic system*.

[5] Schäfer: *op. cit.*, 686.

[6] *Op. cit.*, I, 723 and 680.

explanation of the vagueness and indefiniteness of localization of organic sensation has found so far no basis in the anatomical peculiarities of the afferent sympathetic neurones. Neither absence of medullation in the peripheral fibres, nor the multipolar type of cell peculiar to the efferent sympathetic neurone, is characteristic of the afferent.

Lastly, with regard to the representation of visceral sensation in the cerebral cortex. In general, no reference of visceral sensation to cortical centres has been attempted. The projection of visceral fibres above the medulla seems to be unknown. Even for cutaneous pain no end-station higher than the optic thalami has been demonstrated. Sollier,[1] however, maintains on the evidence of experimental and pathological cases the existence in the parietal lobe (generally given over to the 'stereognostic' sense) of centres for the stomach, intestines, heart, respiratory apparatus, genital organs, and bladder. Ferrier, on the other hand, names the occipital lobe as the recipient of excitations from the viscera. Further research is obviously in order before the question can be satisfactorily settled. Even if, however, the projection of visceral paths beyond the lower centres of the brain should not be demonstrated, and the inaccessibility of visceral impulses to the cortex (except through borrowed paths, as pain) were admitted, the possibility would still remain that consciousness is not limited to any rigid correlation with the cerebral cortex, but accompanies also the activity of the lower centres, and that even the feebler visceral impulses may thus attain a dim sort of consciousness.

Our survey of the histological and anatomical evidence for organic sensation brings us, therefore, to the following conclusion. Although the number of afferent fibres to the viscera is relatively small, their distribution is wide and the corresponding possibility of sensitivity considerable. While on the whole the probabilities are against any extensive differentiation of organic sensation, the possibility that non-painful sensations of some sort or other may reach consciousness from the viscera remains practically uncontroverted. Further, no insuperable obstacle to successful observation and analysis has been demonstrated in the histological conditions of the centripetal pathway. It remains, of course, to examine the limitations on the physiological side which may possibly be established by a study of the experimental, pathological, and clinical evidence.

B. Physiological. The Conditions and Functions in the Organism of Organic Sensation

1. *Adequate stimuli of afferent nerves in internal tissues.* Among the possible stimuli, mechanical and chemical, the

[1] P. Sollier: Le mécanisme des émotions, 1905, 215-7.

following first suggest themselves. The contact or friction on mucous surfaces of food substances, secretions, excreta, etc., on serous surfaces of heavy or distended viscera; the intercellular pressure from dilated blood vessels, or distended gland cells in certain tissues; the distension of ento- or meso-dermal linings (in the stomach, bladder, ducts, blood vessels, alveoli, etc.) by gaseous or fluid contents (operating probably through the stretching of the afferent nerve fibres of these tissues); and the compression through muscle contraction of the endings in the walls of hollow muscle organs (heart, stomach, certain blood vessels, ducts, etc.). Further, the chemical properties of entering substances (food, air, etc.), the drying of the epithelium of stomach and pharynx, the presence of fatigue or toxic products in the tissues, and the varying consistency and chemical attributes of the blood may be mentioned.[1] Of these the physiologists specifically recognize (on the ground of their efficiency in the production of visceral reflexes) the following: distension of lung tissue in inspiration,[2] mechanical stimulation of food in the œsophagus, stomach, etc.,[3] of foreign or pathological substances in the respiratory entrances and secretory ducts, compression through muscular contraction (effective in vaso-dilator reflexes), the chemical properties of the blood in the cavity of the heart,[4] or distension of the latter, noxious gases at the entrance of the lungs,[5] and chemical properties of food or the acidity of the contents of the stomach (effective in the production of secretion, nausea, peristalsis, and the like).

The question whether these stimuli act upon nerve endings specifically adapted to receive them, or whether they belong merely to the class known as common nerve excitants, *i. e.*, are capable *per se* of acting directly upon any nerve fibre they may encounter, is a difficult one. In the former case the possibility of reducing all the excitants listed to a single closely related class, *i. e.*, mechanical (in which case chemical irritants would act only through direct attack of the nerve substance) or chemical (distension would then represent *inadequate* stimulation through stretching of the axones) may be mentioned, and will be referred to later.

2. *Adequate stimuli of internal sensation.* There is general agreement that the mechanical sensibility of the viscera differs radically from that of the outer integument. There is in the

[1] The fact that the viscera are customarily rated *insensitive* to many of the above has no immediate bearing on the point here under discussion, the excitability of afferent, rather than merely sensory, visceral endings.

[2] W. H. Howell: Text-book of Physiology, 1906, 617.

[3] *Op. cit.*, 645-6, 648.

[4] *Op. cit.*, 501.

[5] *Op. cit.*, 625.

deeper tissues no 'contact' sense homologous to that of the skin. The adequate stimuli of internal sensory nerves are physiological rather than physical; they are pre-eminently functionally or reflexly initiated excitations, whether ultimately represented by chemical or mechanical agencies. The impinging of the outer environment in the form of food, air, etc., is as a usual thing indirectly if at all sensible, *i. e.*, in the form of reflexly excited activities which represent not the objective properties of the stimulus, but the effect of the latter upon the organism. Contact or pressure on the inner mucous surfaces is, for instance, effective only in the reflex excitation through autonomic ganglia [1] (plexuses of Meissner and Auerbach) of secretion of digestive juices, peristalsis of the digestive tract, etc., which in turn are probably conscious only when excessive.[2] An exception to this rule must possibly be made in favor of substances producing distension of hollow organs, and thus directly affecting their intrinsic nerve fibres through stretching, as in cases of too rapid swallowing and the lodging of a morsel of food in the œsophagus, or in over-distension of the bladder, stomach, or arteries.[3] It is to be noted, however, that even here not mere presence or pressure of the disturbing substance, but actual distension of the tubes is apparently necessary, and, further, it may be only the resulting muscular reaction of the walls which comes to consciousness. Similarly, the effects of certain exogenous chemical substances, such as the feeling of suffocation produced by certain noxious gases, or the nausea attending the stimulation of the digestive tract by certain poisonous substances, are probably to be interpreted as representing the beginning of protective activity (contraction of the bronchial musculature, or of portions of the digestive tube preparatory to vomiting). They are further noteworthy as associated primarily with the entrances of the great receptive

[1] These reactions may of course represent in part merely the direct effect of a mechanical stimulus upon plain muscle tissue (see American Text-book of Physiology, 1896, 308), or upon its resident motor endings.

[2] The question whether the afferent neurones of autonomic ganglia have any connection with the higher centres is, as we have already shown, a difficult one. Neurological study has revealed so far merely the passage of a number of medullated afferent fibres through these ganglia on their centripetal course from the epithelium. Since, however, the ganglia are accessible to influences from higher centres, *i. e.*, have efferent connections with the cord, the supposition that they have also some sort of afferent connection does not seem extravagant.

[3] Here, as in the case of satiety, certain possible parietal peritoneum sensation, attending increased pressure of the organs and their contents upon the linings of the body wall, may be responsible for the conscious effect.

systems of the body, and as representative of abnormal or injurious conditions.

This brings us to the real reason for the prepossession in favor of correlating sensation with functional stimuli, which is partly a matter of argument that the consciousness of excessive or harmful activity can alone be of value, partly a matter of observation that in such familiar complexes as fatigue, hunger, colic, cardiac oppression, stuffiness, the nausea of indigestion, it is pre-eminently excessive functional activity or the need for such activity which comes to consciousness.

The fact that the functional activity of the organism is closely associated with metabolic change makes a strong case for those who regard the adequate internal sensory stimulus as pre-eminently chemical. The aptness of this doctrine in many of the above mentioned experiences, and especially in such generalized bodily states as faintness, exhilaration, etc., is manifest. At the same time, the probabilities are strong that a certain amount of mechanical stimulation (either muscle contraction, distension, stretching, or pressure upon parietal peritoneum surfaces) enters into the production of such psychoses as cardiac oppression, stuffiness, nausea, satiety, and colic, as well as into the majority of functional activities. Even such apparently refractory cases as the peripheral sensations characteristic of extreme nervous exhaustion and low blood pressure, and usually referred to hypothetical chemical changes affecting circulatory endings or the nervous system generally, may be reduced to mechanical terms, as Sherrington has demonstrated by referring them to the possible relaxation of certain tissues usually kept turgid by an abundant supply of lymph, *e. g.*, the Pacinian corpuscles. Therefore, although Meumann has proposed the chemical processes or products as the appropriate stimuli of internal sensation, and Kröner has posited an indefinite number of organic products as the immediate agents of felt internal changes, we may side if we like with the physiologist Sherrington in the less fanciful belief that the internal sensory stimulation is effected largely by known mechanical agencies,[1] and that such chemical sensations as do occur are probably cases of 'inadequate' stimulation, analogous to the production of visual sensations through the galvanization of the optic nerve.

However this may be, the limits of sensibility may be defined about as follows. The activity of the internal glands, and of the long middle stretches of the digestive tract, are presumably normally non-sensory. Even in the deep tissues for the sensory character of which physiology or neurology offers

[1] Schaefer: *op. cit.*, 971-2.

some warrant, *i. e.*, certain muscle layers (in the stomach, diaphragm, and possibly the heart), certain serous membranes (dura mater, pericardium, pleuræ, peritoneum parietale), certain epithelial surfaces (bladder, stomach, etc.), the appropriate conditions of excitation are probably rare. In both painful and non-painful stimulation spatial summation is presumably important; for both there is undoubtedly a high areal limen. Further, repetition, prolongation, temporal summation, must play a considerable part, especially in pains, whether the stimulus be mechanical or chemical. Whether because of the absence of the differentiated endings and heightened sensitivity peculiar to the cutaneous and higher senses, or to the lack of development of a comparatively frictionless neurone chain (with few synapses) to the higher centres,[1] the liminal values of the sensory endings, and the sensitivity of the viscera to isolated stimulation, are undoubtedly higher and less acute than those of the skin.

3. *Specific energies in internal sensory nerves; the teleological argument.* The question whether the adequate stimuli of the internal organs differ for different endings, *e. g.*, whether there are specific hunger, fatigue or exhilaration stimuli and specialized sets of nerve endings to receive them, whether there is adaptation throughout to a single stimulus mode (mechanical) or whether the effective stimuli are all what is known as 'inadequate' or 'common' in the external pain system, is intimately associated with the problem of specific organic qualities. In the absence of agreement among physiologists, or of definite psychological evidence, we are thrown back upon the teleological argument, which has lately been advanced in favor of specialization. Meumann, for instance, maintains the probability of a great variety of qualities on the basis both of a chemical stimulation hypothesis and the wide variation in the chemical processes and functions of the different tissues,[2] and of the

[1] The speculations of certain writers who suggest that the neural equipment of the viscera represents (at least in its more central portions) an atrophying system, that we have here certain organic functions the stimuli of which were originally conscious, but with the development of higher sensory and functional interests, and the relegation of vegetative functions to subordinate centres, have dropped to the subliminal, while their neural mechanism has undergone a corresponding simplification, seem hardly convincing. Even if these stimuli or functions were originally conscious, they were probably never in relation with a central mechanism or supreme conscious centre, but represent rather originally autonomic and independent functions which are gradually pressing in and at once acquiring a certain influence among conscious processes, and submitting themselves to a certain influence from higher centres.

[2] Kröner's propaganda for a multitude of chemical stimuli adequate to excite the nervous system as a whole (centrally or peripher-

importance of differentiated qualities if discriminated consciousness of internal conditions is to arise and serve as a warning to the organism. While this sounds not unreasonable, the counter view, that the *sensory* stimuli in the internal organs are essentially alike in character, has a certain cogency of its own. If activity and the need for activity are the primary conditions sensed, this appropriate stimulation might conceivably be mechanical and homologous in all organs, *e. g.*, compression through muscle contraction, or tension and intercellular pressure, varying with vascular dilatation, or with the turgidity of the gland cells in a resting tissue. Further, in so far as Meumann's argument rests upon the need of differentiated qualities as the basis for the sensing and localizing of organic wants, we may suggest that the 'local sign' or individualizing factor of any visceral experience is not of necessity qualitative *sensu stricto*. It may take the form either of an intrinsic peculiarity in the grouping or sequence of the sensations, of an acquired local reference, or of the external associated adjustments in which the internal need finds partial, and reflex or instinctive expression (*e. g.*, the muscle contractions preceding vomiting, evacuation and the like).

The whole problem of specific nerve excitants and qualities comes out especially in the speculations as to the real nature of hunger.[1] Opinion differs as to whether the adequate excitation is diffused and general, operating mainly through the altered composition of the blood, or local and relative to the condition either of the mucosa or muscle coat of the stomach, or of both. Those who are unable to reconcile the conditions of its appearance with their own theories of internal excitation regard it as centrally excited;[2] the majority can explain its peculiarities only by reference to both peripheral and central sources.[3] Those who derive it from contraction of the muscle fibres in the walls of the stomach (Müller, Weber,

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ally, and in its external or internal branches), and represented each by its individual conscious quality, stands in defiance not only of teleological argument and scientific 'parsimony', but also of all accepted views of nerve excitability and specialization.

[1] For the facts and theories cited see: J. Müller: *op. cit.*, I, 530; E. H. Weber: *op. cit.*, 145; A. Bain: Senses and Intellect, 129; H. Helmholtz: *op. cit.*, 48; A. Goldscheider: Gesammelte Abhandlungen, 1896, I, 46; Kröner: *op. cit.*, 59; W. Nikolai: Ueber die Entstehung d. Hungergefühls, 1892 (reviewed in Zeit. f. Psych. u. Phys., 1893, V. 358); H. Beaunis: Sensations internes, 24-35; Schaefer's Text-book of Physiology, II; C. S. Sherrington: 991-3 and E. H. Starling, 321; W. H. Howell: Text-book of Physiology, 1906, 267-8.

[2] Z. Oppenheimer: Physiologie des Gefühls, 1899, 36 ff.; E. Becher: Ueber Sensibilität d. inneren Organe, *Zeit. f. Psych. u. Phys.*, 1908, XLIX, 358.

[3] See Beaunis' discussion.

Bain) are inclined to identify it with the muscle (or pressure) quality proper; those who trace it to the mucosa (Külpe, Goldscheider, Sherrington) recognize the possibility of specific endings, specific stimulus, and sensation quality, though inclined to relate it closely to cutaneous sensation.

The physiological facts, drawn from observations in normal and abnormal cases (fistula, fever, etc.), from X-ray examinations and experiments upon animals (cutting of vagi or sympathetic, introduction of nutriment into the duodenum, or of non-nutritious material into the stomach, œsophagus, etc.), are briefly as follows. The stomach in a fasting or hungry animal is empty, anæmic, and at rest in a state of slight tonic contraction, the mucosa being thrown thereby into folds. The sensation which arises under these conditions is temporarily banished by the introduction of any bulky substance into the stomach, or by pressure from the outside; if normally satisfied, it passes over into the feeling of satiety. Further, if not appeased, hunger may after a time disappear of itself, usually to reappear later in intenser form; or its appearance may be considerably advanced or deferred by alteration in the meal hour and habits of the individual. Lastly, dogs evince signs of appetite with both vagi cut, and with the sympathetic connections intact or severed.[1]

Beaunis after a critical consideration of these facts is inclined to suspend judgment and attribute hunger to both central and peripheral causes, the latter to include the condition of the stomach, œsophagus, pharynx and masticatory muscles. Sherrington regards the tension of the mucous lining resulting from local alterations in the blood supply, or the intercellular pressure arising from the engorgement of the cells of the mucosa with digestive granules as all important. The explanation of Goldscheider seems, however, to do most justice to the gnawing, uneasy, fluctuating character of the sensation as usually described. Goldscheider explains it as an irregular pressure sensation analogous to that produced in a finger from which the blood supply has been partially cut off, and in which, owing to the local anæmia, the separate pulsations of the blood are vaguely and unpleasantly sensible. This or some similar use of the fact of anæmia (such as the chemical action on the nerve endings of a diminished or impoverished nerve supply) seems to the writer the most satisfactory; especially since the relief of hunger is always associated with conditions (active peristalsis, introduction of bulky substances, of water, or of

[1] Doubt may, however, be expressed as to whether in the latter case the feeling of hunger itself is present, or merely a co-ordinated eating reflex and its accompaniments.

certain irritants, or predigested substances), the immediate effect of which would probably be either local hyperæmia or a general alteration of the consistency of the blood.

While some writers suggest hypothetical products of oxidation in the muscle tissues or blood as the peculiar stimulus of hunger, no organic substance is known which when injected will produce the sensation. It would hardly seem necessary, therefore, after a survey of these facts, to assume a specific hunger apparatus (and quality) beyond the presence of sensory endings in the stomach so situated as to be especially exposed to mechanical or 'common' and 'inadequate' stimulation through drying or intercellular conditions in general. The consideration of hunger, therefore, adds little weight to the doctrine which would make the internal sensory nerves responsible only to differentiated (chemical?) stimuli. Their accessibility to 'inadequate' or 'common' stimulation or their uniform adaptation throughout the viscera to some form of mechanical excitation (compression?) would, for all we can see, be equally satisfactory.[1]

As to the biological sanction for the existence of visceral sensations (apart from the question of specific qualities), we must agree with Meumann that bodily feeling undoubtedly performs a function in securing concentration on the pressing organic needs of the moment,[2] even if, with Becher, we must admit that this function may be in large measure performed by sensations not 'organic' or 'visceral' in the strictest sense.

4. *Origin and differentiation of the organic contribution to emotion.* One of the principal objects in examining the physiological evidence is to determine the possible scientific basis for the conscious activity of some specific organ (heart, liver, kid-

[1] The writer was at first inclined (on the basis of personal observations) to identify the sense quality of hunger as muscular. Laboratory investigations have, however, shown that the quality usually characterized as 'muscular' is by no means peculiar to muscular tissue or contraction but may be obtained by the application of pressure over almost any surface or structure. In view of this fact and of certain others relating the appearance of hunger to the condition of the mucosa, and its absence in disease to pathological changes in the latter, it seems unnecessary to hold to a muscle theory pure and simple, based, as such a one must be, either upon some hypothetical chemical substance, or very doubtfully upon the quite moderate tonic contraction of the muscle coat, since the absence of active peristalsis antecedent to the beginning of digestion is established by modern physiology.

[2] Even the internal distress of slightly abnormal conditions (such as insufficient or injurious diet, unfavorable climate, etc.), usually cited as a positive disadvantage, may possibly, in the opinion of the writer, operate not unbeneficially in primitive conditions, by inducing a restlessness which may lead to change of habit or habitat, and thus possibly to amelioration of conditions.

ney, or the like) in association with some particular emotion—grief, anger, fear, etc.[1] With the data of physiology and of immediate experience at our command, the direct effect of mental processes upon the great reflexly or semi-reflexly governed organic mechanisms, along with the subsidiary effects upon the glands, capillaries, and unstriped muscle fibres of the skin may be roughly sketched.[2]

Of these no doubt the larger number appear primarily as changes in the external muscle or cutaneous system, as alterations in the activity or tonic contraction due to increase or decrease in innervation of the large body or respiratory muscles, or of the unstriped muscle layer of the skin (gooseflesh, etc.). Others, representing primarily internal changes (vaso-motor?), may nevertheless come to consciousness only through their effect upon the sensory nerves of the cutaneous or body wall system (as tingling, temperature, shudder, etc.). On the other hand, changes in the great body muscles may have a secondary sensory effect upon the serous linings or viscera, while a certain fraction of the bodily response undoubtedly finds expression in the direct inhibition or reinforcing of the activity of the latter.

In general, mental events, acting now as stimulants, now as depressants, upon this centre and that, may give rise to the following bodily changes and probable sources of sensation. In the respiratory system, to violent expulsion or suspension of the breath, and frequently prolonged contraction of certain of the expiratory muscle groups, thoracic and abdominal, involving compression of the viscera, or pressure upon the serous linings of the two cavities, along with altered conditions of cardiac activity; or to increased capacity or depth of the excursions of the diaphragm, and a resultant quickening of the activity of the affected viscera (thoracic especially), along with various remote effects from the increased rate of oxidation in the tissues, heightening of innervation, etc. In the heart, to inhibitory action, followed by increased dilatation in diastole and pressure on the pericardium; or to violent contraction, increased carotid pressure, choking sensation in the throat, etc. In the circulatory system at large, to constriction (possibly even painful) of the cutaneous arterioles, attended by dilatation in the cortical system and a corresponding rise of pressure on the sensory endings of the dura mater, possibly also by sensory effects due to the rise of pressure in the great arteries; to dila-

[1] An ancient doctrine, the modern version of which is perhaps to be ascribed to J. Müller.

[2] See C. Lange: Ueber Gemüthsbewegungen, 1887; A. Mosso: Fear; W. James: *op. cit.*, II, 478-9; Th. Ribot: Psychology of the Emotions, 1898.

tation, flushing, and changes of temperature in the cutaneous system; or to secondary vaso-motor effects (probably due also to direct secretory reflexes) upon the lachrymal, salivary, urinary, and sweat secretions, along with the sensations not improbably resulting from disturbance of each of these systems.

Further into these hypothetical changes, physical and conscious (and their possible classification as purposive or merely irradiatory phenomena, resulting from casual overflow of stimulation in sensory or motor centres), we need not here go. It is noteworthy, however, that physiological data so far fail to suggest any basis for strongly differentiated reactions, involving entirely different organs in isolation, in the case of different emotions or feelings. As Féré and others have pointed out, bodily effects are classifiable mainly as sthenic and asthenic, as furthering or retarding the vital functions *en masse*. It is, of course, quite possible that though differentiated merely as stimulant, depressant, and possibly suspensory in action, the various affective states in virtue purely of a difference in the intensity of their inhibitory or excitatory powers, may call into activity a different range of organs according to the susceptibility or exposure of the latter, and thus give rise to a conscious differentiation. In general, however, while heart sensations (cardiac oppression) may doubtfully be classed as characteristic of depressing emotions; respiratory (or general?) sensations, of exhilarating; and general innervation sensation, of the intermediate exciting and soothing emotions, the basis for any finer differentiation of the internal response is indecipherable in the present state of our knowledge.

C. Clinical and Pathological Evidence.

From the surgeons and experimenters who antedated the general use of anæsthetics (Haller, Bichat, etc.) has come down the doctrine that the internal tissues are insensitive except in case of disease or inflammation.[1] On the limited significance attaching to an insensitivity based on data so restricted we have already commented. The question might indeed be dismissed for the present on the ground that men such as Helmholtz, Müller, Beaunis, Sherrington and d'Allonnes freely admit the normal presence in consciousness of a variety of sensations from the internal organs. Since, however, the dividing line between internal and external sensation has never been accurately established, the possibility of error or illusion in the identification or observation of the former must be admitted. As a matter of fact, the interpretations which certain

[1] *Cf.* E. A. Schaefer's Textbook of Physiology, Vol. II, pp. 970-71; and W. Nagel, Handbuch der Physiologie des Menschen, III, p. 699.

surgeons and clinicians have placed upon the insensitivity of the internal organs in operation contain important implications not only as to the plausibility of a plurality of organic qualities but further as to the trustworthiness and feasibility of observations on organic sensations in general. A closer scrutiny of the facts and hypotheses of this group of workers seems therefore desirable.

The work of the three surgeons (Lennander, Head, and Mackenzie) which we shall here examine deals exclusively with pain; and, further, with the pain of abnormal or diseased conditions. Indeed, the main problem grappled is just the reconciliation of the occurrence of pain in disease with the (assumed) insensitivity of the viscera in health. If the normal afferent impulses are merely reflex in function, possessing no means of access to the higher centres, if in health the internal tissues are proof against all ordinary mechanical, thermal, electrical or chemical stimuli, what can be the conditions of stimulation, what the nervous mechanism by which in case of disease or inflammation a pain excitation is propagated to the cortex? How explain the development or persistence of a pain mechanism or pathway which functions rarely, perhaps never, in the lifetime of the individual?

Certain physiologists and psychologists[1] seem inclined to find the solution of the riddle in the doctrine of semi- or sub-conscious organic sensation always with us but pushing into the focus of consciousness only when greatly intensified, as pain.[2] The Swedish surgeon Lennander,[3] however, scorns any such subterfuge and cuts the Gordian knot by practically repudiating the possibility of any visceral sensation whatever, in health or disease. The data on which this bold solution of the dilemma is based are the result of an extensive and varied experience with surgical cases in which a local anæsthetic only was used. The observations cited cover the testing of a large number of the abdominal viscera, in a state both of health and disease, of rest and of violent contraction. No sensitivity was in any case encountered, *unless* the stimuli employed affected the parietal peritoneum either directly or indirectly. The

[1]M. Foster: Textbook of Physiology, IV, 284; and G. T. Ladd: Psychology Descriptive and Explanatory, pp. 175-6. Ladd, however, is not immediately concerned with the problem of pain, but rather with the 'feeling' function of the sympathetic. See also R. Lagerborg: Das Gefühlsproblem, for the theory that only change is sensed.

[2]Such an hypothesis assumes, of course, that conditions requisite for the production of pain are inherent in any sensory mechanism, ignores the need of a specific set of pain nerves, or a special pain pathway.

[3]K. G. Lennander: Beobachtungen über die Sensibiltät in der Bauchhöhle, *Mitteilungen aus den Grenzgebieten der Medizin und Chirurgie*, 1902, X, p. 38. See also references in Nagel, *op. cit.*, p. 700.

latter responds with pain to light contact or palpation as well as to stretching, and one case at least is noted in which the patient was able roughly to locate the sensation thus produced as upon the right. Neither pain, touch, nor temperature sensations, Lennander therefore concludes, are ever initiated in the nervous tissue of the viscera proper: the sympathetic fibres which supply the internal organs are vegetative or reflex in function merely.[1] Direct stimulation of the cerebro-spinal fibres in non-visceral tissues is requisite to the production of sensation. The pain sensations of operation or inflammation which are usually attributed to the internal organs arise really from the accidental stretching or infection of the parietal peritoneum, pleura, or diaphragm (through the excessive peristaltic action in the adherent viscera, or the spread of noxious chemical products from inflamed and closely associated tissues), and the resulting stimulation of nerve fibres (intercostal, lumbar or sacral, etc.) running in or immediately beneath the structures mentioned. The findings of other authorities[2] are believed by Lennander to be in general reconcilable with this hypothesis. Many surgeons have noted the sensitivity of the parietal peritoneum, while most assertions of the sensitivity of the viscera in disease are matters of inference merely. The classic citation of the insensitivity of the parietal peritoneum rests merely upon a discredited eighteenth century experiment of Haller's upon animals, in which the structureless and nerveless character of the pertioneum is assumed from the start and no attempt made to keep the nerve connections intact in preparing for the experiment.

In criticism of Lennander's sweeping rejection of the possibility of any pain, pressure or temperature sensation from the viscera proper, Meumann in a recent article has raised the following objections: the limitation of the observations to experimentation with artificial and inadequate stimuli, and the unfavorability of the conditions to detailed and accurate observations, *i. e*,, the improbability that under the excitement of operation any sensation short of severe pain could have made its way to the perception of the subject.[3] With regard to the inadequacy of the stimulation we have already raised a

[1] *Op. cit.*, p. 104.

[2] *Op. cit.*, pp. 50 ff. For corroboratory evidence, see also J. F. Mitchell, Local Anæsthesia and General Surgery, *Journal of the American Medical Association*, XLIX, 3, 1907, p. 198.

[3] E. Meumann: Zur Frage der Sensibilität der inneren Organe, *Archiv für d. gesam. Psychologie*, IX, 1907, p. 26.

He also places the weight of certain *a priori* arguments, and of his own observations and introspections, in favor of the probability of a varied continuum of sensations from the internal organs, as already noted.

further point. The rôle of summation in internal excitation, recognized by the physiologists but totally ignored in the inferences of Lennander, may in part explain why such stimuli as stretching, or intensive contraction of the muscular wall of the alimentary canal, which we have every reason to think represents a normal source of sensation or stimulation, should be as ineffective as cutting, contact, and thermal applications.

In attempting to evaluate Lennander's conclusions, it may be useful to point out the two sources of his bias in favor of the total insensitivity of the viscera: the insensitivity in surgical operations to pain of all tissues (so far as tested) except those immediately associated with the body wall and diaphragm; and a prepossession in favor of the co-ordination of sensory function with cerebro-spinal nerves. With regard to the latter, it is noteworthy that in acknowledging its influence on his choice of the subserosa (which is innervated from spinal nerves) as the sensory tissue of the abdomen, he apparently ignores the cerebro-spinal fibres which Langley and others have traced into the viscera along sympathetic pathways. As to his results in general, regret must be expressed that more careful records of the apparent localization of the pain following the stimulation of different parts of the peritoneum was not kept. Finally, we may again suggest that even though much of the pain of disease, and probably the greater part of that incident to surgical operation, arises in the fashion prescribed by Lennander, this admission leaves the problem of the actual seat of origin of the various internally referred experiences of normal life practically untouched.

The position of Mackenzie is only in small measure less extreme than that of Lennander. According to the former, all internal reference of sensation is an illusion, an error of judgment; the body wall (muscle, skin, and subserosa) alone is sensitive. The afferent pathways of the viscera do not extend to the cortex. In so-called visceral pain, the nerve impulses may indeed be initiated in afferent fibrils of the unstriated muscles of the internal organs (never in mucosa or serosa even in case of disease or inflammation), but they attain to consciousness only by transference to cerebro-spinal pathways and centres belonging properly to the body wall. This conclusion is based in part upon the static character of the pain referred to certain organs known continually to undergo changes of position, such as the heart and stomach, in part upon the discrepancy found to exist between the limits of the spontaneous pain of disease (as defined by palpation in diagnosis) and the limits of the diseased organ or tissue itself as revealed in operation. In view of these facts and the marked sensitivity or hyperalgesia (as tested by pinching between the fingers) of

the body wall over the affected organ or thereabouts, he assumes that 'visceral' pain is mediated by irradiation from afferent neurones of the viscera to sensory neurones supplying fibres to the skin, muscles or subserosa[1] from the same segment of the cord, and is accordingly referred to the same general region of distribution as the latter.[2] The superficial tenderness of disease is merely another phase of this irradiatory action and is indicative of the hyperexcitability to which the neurones of the external system have been raised by continual irritation from visceral fibres.[3] That is, Mackenzie reconciles the insensitivity of the viscera during surgical manipulation with the apparent sensitivity of diseased organs in diagnostic handling by declaring that in the latter instance only somatic (body wall) nerves are affected. Afferent impulses capable of terminating in pain can apparently be excited in the viscera by no artificial means unless it be stretching. Their adequate stimulus is prolonged or excessive contraction.[4] The sensitivity of the serous membrane is denied on the basis both of clinical cases and of the poverty of nerve endings in that tissue, a dictum apparently widely at variance with that of Lennander. Since, however, in the second article quoted, Mackenzie admits the sensitivity of the subserosa along with the skin and muscle of the body wall, his earlier statement is not necessarily unreconcilable with Lennander's insistence on the pain function of the parietal peritoneum.

On the purely afferent and non-sensory function of the sympathetic fibres Mackenzie is in his way as dogmatic as Lennander. Pain is a function of cerebro-spinal nerves only. It is always 'somatic', never 'splanchnic', as its reference in any given case to the general area of distribution of the cerebro-spinal nerves which would conceivably be affected through irradiation bears witness. Its internal reference, when this

[1] Mackenzie: Meaning and Mechanism of Visceral Pain, *Brit. Med. Jour.*, I, 1906, pp. 1449-54, 1523-28.

[2] This explanation of the nature and mechanism of 'referred' or transposed pain is really a development of the irradiation hypothesis for 'sympathetic' pain put forward in the rough by J. Müller (Elements of Physiology, tr. by Baly, 1837, Vol. I, p. 723), and first definitely applied by J. Ross (The Segmental Distribution of Sensory Disorders, *Brain*, X, 1887, p. 333).

[3] It is interesting to note in this connection that the fact of hyperalgesia is apparently reconcilable only with the theory which accords a common peripheral apparatus to touch (or temperature) and pain, accounting for the latter by the hypothesis of a secondary high resistance conduction path in the gray matter of the cord. The doctrine of specific pain endings with high limen of stimulation would apparently offer no basis for this lowered sensitivity.

[4] *Brain*, XXV, 1902, p. 368.

takes place, is essentially illusory.[1] The adaptive or purposive nature of this device (*i. e.*, the transfer to body neurones and the substitution of body wall hyperalgesia or pain for visceral pain) he argues from the fact that the hardening of a portion of the body wall, reflexly effected in consequence, undoubtedly serves as a protection to the affected tissues situated actually or approximately beneath.[2]

Such a theory, denying as it does the existence of any private pathways from viscera to cortex, with central stations of their own, would, if accepted, cut away the ground from the hypothesis of specific organic qualities, hunger, nausea, suffocation and the like. Even though the transfer of non-painful as well as painful impulses were conceded these must, according to accepted theories, appear in the qualitative guise peculiar to the borrowed paths or neurones.[3] Several points may, however, be raised against the unconditional acceptance of Mackenzie's line of argument. First, in denying sensory functions to the sympathetic on the score of the fewness of its fibres in proportion to the bulk of the viscera, and the other and varied functions which these must subserve in the vegetative economy of the organism, he ignores both the possibilities of intensification through spatial summation provided by the sympathetic, and the presence throughout the system of cerebro-spinal fibres. Secondly, the peculiarities of local reference adduced in support of his conclusions are in the main handled on the basis of a psychology of space too naïve and ambiguous to be convincing. For example, in proceeding

[1] A curious instance of an illusion which represents the objective if not the immediate subjective truth. The seat of the disturbance is pretty correctly sensed, but the significance of the route by which it reaches the cortex is ignored. It may, moreover, be suggested that the secondary pain path in the cord which Mackenzie evidently assumes may just as well be conceived of as belonging to the internal as the external system, and it may be regarded as only more or less of an accident that the local signs of the external system customarily adhere to its stimulation. In this case the insistence on the illusion of internal reference loses much of its force.

[2] The displacement of the viscera from their original segmental position in the course of mammalian development is called upon to account for the occasional discrepancy between the position of the diseased organ and the localization of the pain or the section of the wall contracted.

[3] The query might be raised in this connection as to why leakage to temperature and pressure as well as pain neurones does not take place in the cord, *i. e.*, why visceral disturbance is not sometimes sensed as heat, cold, or temperature. Mackenzie would, however, probably answer that every sensory neurone has a high resistance gray matter conduction path correlated with pain, and that impulses intensive enough to escape from visceral to body wall neurones are also intensive enough to effect the passage of this path, to the prejudice or swamping of the other possible sensations.

from the fact that the abnormal position of an organ is not correctly sensed to the conclusion that the seat of all sensation must therefore be in the stationary body wall, he ignores the customary mechanism of localization, *via* associations, visual, motor or the like, based upon the accumulated data of experience, and holding good for normal or typical cases only. The localization of pain in a constant position with reference to the body wall, though the actual situs of the organ excited may have altered, is a phenomenon resting conceivably upon the same basis as the reference to an amputated limb of sensations arising from the excitation of the nerves of the stump. Further, in the case either of stationary or moving organs a certain amount of vagueness or inaccuracy must always be allowed for in view of the peculiar conditions and the lack of incentive to the establishment of exact processes of localization in the viscera.[1] Mackenzie, however, habitually speaks of space perception in terms of immediate intuition. Again, the reference of internal sensation toward the anterior body wall, which is indeed commonly noted,[2] and which might appear to support Mackenzie's thesis, may be often merely a matter of the attraction of the sensation to be localized outward, toward visualizable or familiar boundaries, rather than inward toward an unexplored third dimension. Moreover, the fact of body wall hyperalgesia or pain is in itself no argument against the simultaneous presence of true visceral sensation, as indeed Head himself virtually admits. Lastly, no positive evidence against the direct conduction of sensory impulses to the cortex from the viscera by visceral neurones is adduced by Mackenzie. This point is merely inferred from the assumed incapacity of the sympathetic for pain.

Head,[3] in his article on visceral disease, occupies a less radical position than either Lennander or Mackenzie, admitting as he does that the viscera are veritably sensitive, mucous, muscular, and serous surfaces alike. Pain both originates and may be felt therein without involving an 'error of judgment'.

[1] This same criticism does not, of course, apply to Mackenzie's citation of the decided regional separation of reference in the pain from the large and small intestines (*op. cit.*). If his statement that pain from the large intestine is confined to the region below the umbilical area, while pain from the small intestine is customarily referred higher, is sustained by the observations of others, some extra-visceral explanation of the severe pain of visceral disease must evidently be sought, whether in terms of his own system or of that of Lennander or Head.

[2] This peculiarity has, as a matter of fact, been noted by both Helmholtz (Tatsachen in der Wahrnehmung, p. 48) and Meumann (*op. cit.*, p. 52).

[3] H. Head: On Disturbances of Sensation with Especial Reference to the Pain of Visceral Disease, *Brain*, 1893, XVI, 1-333.

Such pain is, however, of the dull, diffuse, vaguely localized variety, akin in every way to the 'protopathic' type of cutaneous sensation which Head in his article on the injury of the nerves of the arm has separated out from the sharply defined and located 'epicritic' sensitivity of the same region. For this dull internally referred sensation of the viscera either sympathetic endings or cerebro-spinal fibres terminating in Pacini corpuscles are responsible,[1] and in its mediation presumably only visceral neurones are concerned. The great bulk of visceral pain is, however, of the sharp, stabbing, clearly defined variety, and is felt in the cutaneous region innervated from that segment of the central system from which the nerve supply of the organ affected is also derived, owing to the transfer of the afferent impulse from visceral to cutaneous neurones in the cord. In support of this conclusion, which in itself, of course, represents little new, he cites (and here lies his importance) a tremendous system of evidence drawn from detailed mapping of the areas both of tenderness (hyperalgesia) and of severe pain attending affections of the various viscera, together with similar observations upon the skin areas affected in herpes zoster at different levels of the cord. The observations for both herpes zoster and hyperalgesia show irradiation in non-overlapping bands around the body; peritoneal and serous affections alone constitute an exception to the rule. Further, the points or areas of severe pain lie usually in the corresponding hyperalgesic regions, and frequently at some distance from the tissue diseased.[2]

In this theory, as in the doctrine of Mackenzie, the irradiation of nerve impulses posited offers no especial difficulties. Its operation would indeed be essentially similar to that of the mechanism assumed in the ordinary theory of a gray matter conduction path for the pain of the external system. The difference is mainly that where the latter assumes for any peripheral neurone a direct route for non-painful impulses, an indirect one for pain, the hypothesis of Head practically maintains two pain paths to the cortex, a primary private path, and a secondary one borrowed from the external or cutaneous sys-

[1] The Afferent System from a New Aspect, *Brain*, 1905, XXVIII, 99-113.

[2] The facts that the areas mapped are not appreciably overlapping, and that gaps in cutaneous reference correspond precisely to the regions supplied by the brachial and lumbar sections of the cord, which Langley has shown send no fibres to the viscera, are cited in favor of the hypothesis that the irradiation takes place in the *cord* and not in the spinal roots, each of which may be derived from several adjacent segments of the cord and which represent irregular and overlapping peripheral fields.

tem.[1] The external secondary path theory assumes either that the centripetal impulse expends itself in forcing the high resistance path and in the production of pain, or that the excitation travelling along the primary pathway reaches consciousness also, and is sensed in its appropriate terms, although frequently swamped by the pain component approaching along the secondary path. This second alternative is evidently that adopted by Head for visceral sensation. He does not actually deny the existence of a true visceral pain simultaneously present and parallel with the sharp 'referred' skin pain, but this if present blends with the latter, is *felt* in the skin. That is, there is not only a physical transference of visceral impulses, and a corresponding projection, but also a psychical misreference of true visceral pain.[2] This peculiar state of affairs is, however, quite adequately explained by Head as analogous to the allocheiria which arises in the external system under certain abnormal conditions entailing reduced sensitivity of certain regions, and which is definable as the reference of a sensation to a more sensitive asscciated area (adjacent or symmetrical). The terms in which Head phrases the application of this principle to visceral pain are somewhat obscure, but the process itself seems readily explicable on the ground that the cutaneous nerve impulse or corresponding cortical end process commands a more definitely organized body of assimilating and localizing associates, and, further, possesses a more efficient directive power over these associates (in virtue perhaps of its intrinsic energy or dynamic intensity), than the feebler nerve processes in the organic channels proper. Hence in cases of relatively intense visceral excitation with irradiation into cerebro-spinal channels, the perceptive associates controlled by cutaneous neurone processes swamp those of the less vigorous organic components which gave them origin. In the words of Head, the 'diffusion area' is accepted by consciousness by an 'error of judgment'.

If we grant, as we must, the essential reasonableness of Head's hypothesis, the desirability of harmonizing his observations with those of Lennander at once arises. Unfortunately, however, the two sets of data are taken from too widely different points of view to be readily comparable. In the absence among Lennander's observations of any detailed correlation of

[1] This peculiarity apparently arises from the fact that Head favors specific pain nerves rather than a secondary path theory for both the external and the internal system (*op. cit.*).

[2] That is, Head's statement seems to indicate that it is the corresponding localizing associations or cortical processes rather than the visceral pain itself which fail to rise above the limen of consciousness when accompanied by referred pain. *Cf.* Disturbances of Sensation, *Brain*, 1893, XVI, 124-6.

the objective and subjective sites of disturbance, *i. e.*, of the precise location of the portion of the peritoneum affected and the localization of the accompanying pain (by the patient), the weight of authority must rest with the elaborately systematized and coherent results of Head. The likelihood that if Lennander's denial of the sensory functions (direct and indirect) of the sympathetic system were accepted, the subserous parietal neurones could be substituted for visceral proper as the source of irradiation to cutaneous neurones, without doing violence to the system of facts accumulated by Head seems slight. If Head's referred pain of the hyper-sensitive bands were to be ascribed to stimulation of the intercostal or lumbar fibres running in or beneath the parietal peritoneum (to supply the body wall) and external reference of the corresponding excitation through either irradiation, or peripheral projection, it would be difficult to explain such facts as the reference of visceral pain higher or lower according as the cardiac or pyloric end of the stomach is affected, since presumably in either case the entire gastric connection would be concerned. This difference in reference might of course find a basis (in terms of the theory of Lennander) in the probability that affections of the cardiac end of the stomach involve the peritoneal connections with the diaphragm, affections of the pyloric end, the lower and dorsal mesenteric attachments of the duodenum. Similarly, Mackenzie's instance of the higher and lower reference of pain according as it originates in the small or large intestine might be explained by recourse to sites of the corresponding peritoneal connections—mesenteric in the former case, lower caecal or sigmoid flexure attachments in the latter. So far as the bandlike hyperalgesic areas are concerned, however, these according to Head could never be assigned to peritoneal stimulation since the pain arising from the parietal peritoneum (*e. g.*, in cases of inflammation) is never referred (*i. e.*, never transferred through irradiation to cutaneous neurones), and is irregular, not bandlike, in its manifestation; that is, such diffusion of sensation as does occur is a matter not of irradiation but of peripheral projection of sensations resulting from the stimulation of fibres somewhere on their centripetal course. It seems on the whole, then, probable that while Lennander's explanation is valid for certain active and griping pains such as those of colic, Head's holds for certain other semi-pathological hyperalgesic or referred pains. At the same time the possibility of genuine resident pain or normal sensation in the viscera is not disposed of.

This brings us at once to the question of the bearing of the hypotheses of the pathologists upon the problem of the localization and sensing of normal sensation (if this may be

granted to exist). What explanation, for instance, does each system make possible of the off-hand local reference of unpainful as well as painful organic sensation, what possibilities of illusion in regional localization as well as in inward reference does each reveal?

The popular notion of localization is of course that the seat of hunger, nausea, or internal pain is either immediately and intuitively sensed, or may be ascertained by means of exploratory pressure from without. The important rôle, however, which such suggestion, and an atomical or physical knowledge may here play (see for instance the wide divergences in the localization of hunger in the case of the educated and the uneducated reported by Beaunis) and the uncertainty and instability in the mechanism of localization thereby indicated would seem too great to warrant the application of any nativistic doctrine. With regard to the second alternative, localization by means of palpation is indeed abstractly conceivable, if external pressure be admitted as effective in either setting up, diminishing, enhancing or putting an end to the particular sensations concerned, as immediate experience would seem to affirm, and as indeed Meumann has assumed, without any apparent sense of the contradiction involved with his principle that functional changes alone are the adequate stimuli of sensation. Or, localization might be empirically effected by evoking certain definitely localized muscle or skin sensations which in previous experience have become so closely associated with or assimilated to the internal sensations in question as to represent their local sign, as Helmholtz suggests.[1] In either case, however, we find ourselves immediately involved in the necessity of reconciling the setting up of said alterations in internal sensation and the formation of associative local signs out of external sense material, with the pathologists' doctrine of the insensitivity of the viscera to all mechanical stimulation.

Irrespective of any theoretical limitation of the sensitivity of the viscera, the following possibilities may be suggested. First, while there are indications that pressure does not represent the adequate *pain* stimulus of the viscera, it may nevertheless excite certain of their nerve endings to a sensible if feeble and non-painful action,[2] as it undoubtedly does those of the parietal peritoneum. Apart, however, from this possibility of direct action, it seems reasonable to assume, whatever theory of the extent of the sensory and non-sensory tissues we

[1] *Op. cit.*, pp., 47-9.

[2] If as Sherrington maintains (see preceding section) the adequate internal stimuli are largely mechanical (in the form of compression, not contact) there is a bare possibility that in certain cases pressure from without may be feebly if not painfully effective.

may entertain, that such mechanical stimulation affects certain of the *afferent* endings of the viscera, and may thus reflexly, under favorable conditions, through the resulting changes in visceral muscles, evoke sensations. Even more probable is it that through the displacement of gaseous or fluid contents external pressure may produce alterations in visceral distension, and thus give rise to sensation either directly through the stretching of afferent visceral fibres or increased parietal pressure, or indirectly through the compression of the resident endings, or the stretching of subserosal parietal nerves, attendant upon reflexly increased vsceral activity. Secondly, with regard to localization through associated sensations or movements. It is, for example, possible that the discharge of the respiratory function may in certain cases be effective in altering sensation, either directly and mechanically through displacement and pressure (in some such way as that suggested above), or indirectly and remotely through the metabolic changes which it induces in lung and other tissues. The external or kinæsthetic sensations thus associated to certain of the internal sensations may come to act as the 'local sign' of the latter and exert a certain directive power over their localization even when arising from different causes. Thus intestinal pain, nausea, and hunger may conceivably get their general reference from the external sensations representative of diaphragm contraction or abdominal wall distension or contraction; the feelings of stuffiness or exhilaration their thoracic or cephalic reference through association with chest muscle contractions, or with the constrictions or relaxations of the nasal passages and respiratory entrances. The possibility that the sensations thus associated may represent conditions standing not at all in causal relations, *e. g.*, in the association of sensations due ultimately to general circulatory changes with respiratory movements, of course arises. Further, even where a direct causal connection exists there may be a considerable discrepancy between the actual seat or origin of the sensation and the locality indicated by the external localizing movement. These points come out more strikingly when the localization of pain is examined from the point of view of each of the theories under discussion.

Take, for instance, the theory of Lennander that the peritoneum parietale, pleuræ, etc., are alone sensitive. What becomes of the apparent spatiality of organic sensation, of the possibility of determining its seat in the organism by external palpation? In the first place, the so-called massiveness, or voluminousness, the triple dimensionality of organic sensation, is reduced to a hollow mockery. Further, though the parietal sheet of subserous tissue may be directly sensible to pres-

sure and though every portion of this sheet accessible to such pressure from the outside might have its own local sign[1], yet the portion accessible, also, to stimulation from the viscera must be exceedingly small. Indeed, in the abdomen if not in the thorax it must be limited to precisely those tracts of the posterior and upper walls which can have no local sign so associated to them. Localization toward the front wall of the abdomen must then represent an illusory reference, to be explained either as representative of some such central irradiation and segmental reference as Head has figured in his theory, as the result of the peripheral projection of sensations resulting from stimulation (in the rear wall) of the trunks of intercostal and lumbar nerves, the endings of which, distributed mainly to the front of the body wall, lend their corresponding local sign to the resulting sensation, or in one of ways suggested in the preceding paragraph. In the latter case, the throwing forward may be merely a matter of association with the sensible activity of abdominal and thoracic muscles or diaphragm in respiration, with the pull of diaphragm contraction upon the anterior wall, or the increased pressure of the body against the clothing during inspiration. A basis for the formation of such associations is further afforded by the fact that the pain from the drag of the stomach on its peritoneal connections would conceivably be lightened by lowering the diaphragm, renewed again when it lifted, while in the case of painful pressures upon the peritoneum the opposite would hold. In any case, however, within the theory of Lennander only the most general reference of internal sensation would be possible. Correlation with the precise organ active in its origin would be little short of a miracle.[2]

For Mackenzie, the localization of internal sensation is simple enough. It is felt always in the body wall, in accordance with the principle of irradiation from splanchnic neurones and peripheral projection, each internal tissue having a definite correlation with some patch or portion of the external body wall. For him, also, exploratory pressure produces a sensory

[1] As a matter of fact Lennander leaves us very much in doubt as to whether he regards pain as the result of the stretching (or inadequate stimulation) of parietal nerves running to supply the muscles and skin, or whether he recognizes the presence of *bona fide* sensory endings sensitive to compression or chemical excitation under the epithelial plates of the peritoneal linings. Unfortunately, neurologists can as yet give us no precise information on such endings.

[2] In many of the possibilities suggested, however, to call the misreference in which the localization consists pure illusion would be the merest quibble, since the seat of the main physiological disturbance, if not of the immediate sensory excitation, would roughly coincide with the region of reference.

excitation in the somatic endings only, thereby reinforcing the local sign of the centripetal neurones already excited by internal irradiatory processes. In no case is localization trustworthy in any detail. Further, for Mackenzie as for Lennander, the third dimension is an illusion: the distinction between internally and externally initiated sensation is fairly blotted out.

For Head, the above likewise holds true, with, however, certain qualifications, since, according to him, the duller, weaker sensations of pain are actually localized in the third dimension, though somewhat vaguely. With regard to the possibility of applying his principle of referred pain to the localization of faint or unpainful sensations, an expedient suggested by Meumann, it must be noted here that Head himself does not claim that the localization, such as it is, of dull visceral sensation (for him, always pain) is determined by the same law as that of the sharp referred pain arising from the same endings. In dull pain, the excitation is by hypothesis too weak to effect irradiation; and further, a stronger excitation accompanied by sharp referred pain may conceivably never have been initiated in that particular tissue, thus precluding the formation of association with any somatic region. It is indeed difficult, if not impossible (without abandoning the principle of the insensitivity to mechanical stimulation of the internal organs, or modifying it in some such way as we have suggested), to conceive of any mechanism by which even vague localization of the duller phases of visceral sensation could be set up in experience. Head's own position in the matter seems to resolve itself into an intuitive or nativistic theory of organic (and tactual) space. Although in one passage he expressly states that localization is dependent upon association, his treatment of the concept of localization (in another connection) belies his words. He uses, for instance, the fact that at a certain stage in the regeneration of the fibres in an injured nerve trunk diffuse, indefinite reference of sensation alone occurs, as evidence for the classification of sensory nerves into a protopathic and epicritic set, representing different grades of localizability and possessing different rates of regeneration. This would seem to argue a failure to recognize the complex nature of localization, and the necessity for the gradual reinstatement of the processes disturbed by abnormal conditions before accurate reference can again arise. Much the same naïve attitude toward the psychology of localization and reference is apparent in the writings of Mackenzie, as we have already pointed out.

In summary, we may say that so far as the accuracy of regional reference of *pain* is concerned, it is for Head and

Mackenzie, and possibly also for Lennander, only a happy accident that the region of reference and the underlying seat of origin are in some measure and in certain cases coincident or adjacent. Further, with regard to the possibilities of accurate localization of unpainful sensation (if, indeed, this may be granted to exist) the above theories taken strictly offer little warrant. Even upon a more liberal sensory doctrine either of the tissues sensitive or of the adequacy of artificial mechanical stimuli, the probability is strong that localization (especially in the thoracic regions, where the application of direct mechanical stimuli is even more dubious than in the abdominal) means merely association with the local signs furnished by movements which, while often concomitant, are dubiously causal or relevant. It may indeed be that while almost every organic feeling is capable of an apparently definite local reference, such local reference most frequently is a matter of suggestion or inference merely, and is pure illusion so far as the actual position of the nerve endings stimulated is concerned. Again, with regard to internal reference and the third dimension, the insistence of Head and Mackenzie upon the actual external status of many pains usually regarded as internal suggests the probability that among unpainful as painful sensations many commonly rated as internal or visceral may actually originate somewhere in the external system; that no universally valid criterion of external and internal exists; and, lastly, that the separation of organic and external sensation in investigation is hardly practicable.

While the facts and theories of the pathologists, therefore, help us only a very little way in the study of the possible sources and kinds of organic sensation, they are valuable in demonstrating the need of a critical examination of all rough and ready modes of localization or identification of organic qualities, of the clues by which such localization is effected, and their relative validity. It is, moreover, to be hoped that with the extension of the use of local anæsthetics a growing body of available data bearing upon the localization of pain will supplement the introspective and experimental results obtainable under normal conditions.

Experimental

Our object here, as already stated, was to investigate the richness or poverty of the organic gamut; to determine the traits by which organic is differentiated from ordinary sensation; the possibility of singling out and localizing the organic qualities under the play of attention; and, lastly, to devise a fitting terminology for whatever qualitative strains might be found to characterize the different feeling attitudes, emotions, etc. The

classic difficulties in the way of such a scheme have been already cited; the impossibility of isolated stimulation, shortcomings in the way of memory images and other aids to attentive examination and description, and the inaccessibility in laboratory research of the major emotional and vegetative complexes in which organic sensations presumably play their most striking part. Yet the resources of the laboratory are worth more careful examination. Certain comparatively simple or contracted feeling aspects are there at our command: surprise, expectation, recognition or familiarity, alarm, doubt, amusement, certainty. Further, various *Mitempfindungen* or minor organic reverberations are attendant upon simple sensory stimulations accessible, *e. g.*, in the bodily effects of certain tastes, odors, noises, and the like. In such compounds the occasional emergence of an individual organic quality through accidental changes, differences of intensity, etc., is not impossible. Further, by fractionating the observations so as to fix the attention now upon one part of the body, now upon another, and by facilitating description through furnishing the observer with an extended and standardized list of adjectives, it seemed probable that certain verbal clues and associations could be established by means of which various casual experiences such as hunger, fright, drowsiness, nausea, embarrassment, disappointment, regret, and other semi-emotional complexes might later be laid open to analysis and description.

Organic Attitudes. First of all, we devised a set of simple experiments with the Jastrow drop apparatus and a number of series of inkblot figures, names of well-known men, colors, and numerals (with a certain number of blanks interspersed in the series), so arranged as to provide the appropriate conditions for such mental reactions or attitudes as the imaginative, the recollective, expectancy, surprise, and recognition.

These preliminary experiments were pretty uniformly unsuccessful. The four observers tested failed to report any organic factors, beyond various externalized kinæsthetic strains and relaxations, and alterations in respiration, varying little for the different types of experiences. It was evident either that the bodily attitude of active attention and release therefrom is in laboratory experiments of this type so prominent as to swamp any possible specific organic characteristics of the several attitudes, or that some special development of the technique of observation upon physiological sensations was necessary. We, therefore, abandoned for the time the use of reactions to these comparatively complex stimulus situations, and turned to the study of the simpler, more striking and

varied, and hence more readily describable physical reactions to simple sense stimuli of decided affective value.

Organic Reverberations. A number of tastes and odors were tested for their physiological effectiveness, and the following selected: quinine and sugar solutions, vinegar, lemon, sarsaparilla, laudanum, camphor, mace, ammonia, carbon bi-sulphide, listerine, castor oil, gasoline, asafœtida, ginger. These were submitted to the observer for periods of from five to twenty seconds (at considerable intervals), with the instruction to give himself up to the experience of the moment (*not* to the cognition of the stimulus), to get all the pleasantness or unpleasantness possible out of it, and then to report upon any concomitant organic reverberations, diffused or specific, superficial or apparently internal and deep-seated.

In general, all four observers reported with unpleasant stimuli a feeling of revulsion, traceable to the automatic contraction of the muscles of the mouth, throat, nasal or respiratory passages, and sometimes grading into a feeling of nausea. With pleasant stimulation, either a feeling of relaxation, general or localized in the muscles of the face, chest, or limbs; or a 'glow' felt in the head or referred vaguely to the respiratory apparatus, the activity of which seemed always appreciably involved. Sensations of nausea were referred usually four to twelve inches below the larynx, sometimes to the base of the tongue, and described by all observers as wavelike, muscular, uneasy. Pricking sensations on the tongue, in the nasal passages, or proceeding apparently from the salivary glands, were recorded following stimulation from vinegar, lemon, or ginger, or the whiffing of ammonia. In spite, however, of considerable variety and difference in reference (the stomach, œsophagus, respiratory passages, pharynx, salivary glands, etc., were all apparently at one time or another affected) there seemed to be no necessity for referring in description to any specific quality other than the familiar external categories, unless the mention of 'pricking', which seemed neither pain nor pressure precisely, be excepted.

Organic Mitempfindungen. A third set of experiments had for its object the study of certain borderline phenomena (not visceral, but presumably subcutaneous and distinguishable from pressure, contact, or ordinary muscle sensation), such as tingling, the 'creeping' or gooseflesh sensations, etc. Rasping and harsh auditory stimuli and rough textures were used here. For the sake of contrast a number of pleasing tones or chords were introduced into the series, and their organic accompaniments duly noted. A vocabulary of descriptive terms (mostly, it must be admitted, characterizations of merely temporal or spatial peculiarities), such as tingling, pricking, stinging,

thrilly, aching, glowing, dull, sharp, massive, delicate, vague, creeping, and the like, were, after a few observations, supplied to the observer, and their significance later agreed upon. A very shrill whistle, a bowed Mi_5 fork, cymbals, discords, and a major triad on the König tuning forks were used.

While a considerable variety of organic reactions was thus secured, the introspections again failed to show the presence of any peculiar organic qualities. Among the mass of muscle contractions noted and localized in body wall, limbs, ear, or head region, visceral sensations proper were mentioned only twice, and then doubtfully, as the apparent concomitant of contraction of the abdominal wall muscles. Unpleasantness was sensed mainly as a checking or obstruction of respiration, a general muscular recoil or revulsion, or a positive (localized) pain, *e. g.*, in the ear. Pleasantness was again referred vaguely to the chest or respiratory region. Tingling or waves of shivering, 'creepy' sensations were frequently noted, but not further analyzed or qualified, and hardly internally referred or differentiated from cutaneous sensations.

By this time, then, it was pretty evident either that visceral sensations are lacking in other than strongly toned emotional complexes, or that they are far less easily discriminable from the ordinary external qualities than we had so far assumed. A series of casual introspections on a wide range of spontaneous organic complexes seemed to favor the latter conclusion. Accordingly, in a fourth set of experiments (still more or less crude and exploratory) we set out to work systematically from the known to the unknown, running the observer through a typical series of external sensations—pressure, pain, muscle, etc., and their simple variations—which should then serve as standards of comparison in dealing with the sensations following similar stimulation of the accessible mucosa (*i. e.*, of the tongue, and throat, œsophagus and stomach). Special attention was to be paid to the possible appearance of diverse kinds of cutaneous or subcutaneous and muscle pain; to the comparison of the effects of heat and cold upon mucosa and epidermis; to the analysis of itch, ache, tingle, smart, burn, and other of the simpler external complexes, with a view to the discovery of some new specific element (prick or tingle?) other than the certified pain and pressure qualities, representative, perhaps, of a subcutaneous or circulatory contribution, hence likely to afford a clue to deeper visceral sensations.

Comparison of the Direct Effects of External and Internal Stimulation. Experiments on heat-pain were carried through as follows. Several small areas on the forehead and arm were tested with heated temperature cylinders, the pointed and blunted ends of which were alternately used in order that in

the latter case the appropriate conditions for the stimulation of both warm and cold spots, *i. e.*, for Alrutz' 'heat' fusion, might be present. The stimulus was applied from three to five seconds, allowing considerable intervals for recovery from fatigue. The following is a typical set of observations on the arm with the blunt end of the cylinder.

39° C. *a*. Warm; a little flash of heat toward end.
b. Heat at once, a prickly sharp point, not areal as when merely warm.
41° Warm, then suddenly hot, then warm again.
43° Heat at once, flickers, warmth between.
45° More like heat; never constant.
47° Hot, then pain. Heat flickered, warmth between, pain only with first flash of heat.
51° Heat, changes to pain, but not piercing.

From such observations as these, and further questioning, the fact came out to our surprise that the experience known as 'heat' to our observers is very slightly suggestive of a fusion of warmth and cold. The description of heat as a *point* without the *areal* feeling common to warm and cold spot stimulation, its delayed appearance, and flickering, fluctuating character suggest that the major feature is a weak pain, akin to prick, to 'pins and needles', to the sensations resulting from the application of a mustard plaster, or a weak interrupted current to the skin; of salt, pepper or ginger to the tongue.

For the direct comparison of heat and cold pain one finger was dipped into water heated to 65 or 75° C, another into ice water or the surface chilled by spraying with ether. Under these conditions heat and cold pain were found to differ, though hardly qualitatively. The former is more superficial, sharp, piercing, unsteady, throbbing out particularly strong just after the finger is removed from the water, and dying away into tingling, or soreness. The latter appears less abruptly, is deep, steady, massive, and spreading, especially to joints—wrist, elbow and shoulder—where it persists as a dull ache or strain sensation some time after the removal of the stimulus.

Our experiments were of course too roughly carried out to indicate whether the pain in either case represented over-stimulation of temperature spots (Bader, Wundt), direct action upon pain fibres proper (possibly at different depths for heat and cold pain respectively) or a secondary effect related to vaso-motor changes. In favor of the first hypothesis stands the fact that pain adaptation seems to follow temperature adaptations, *i. e.*, a cooled finger responds painfully to lower temperatures than it would under ordinary conditions. In favor of the second, especially in so far as it concerns the

origin of heat pain in superficial, cold pain in deeper strata of pain fibres, stand the difference in depth of reference of the corresponding sensations, the customary sharpness of heat pain, the aching character of cold, and the frequent adapting out of the former, characteristics which tend to identify it with the superficial pain sensitivity to mechanical stimuli. Whether, however, the painful temperatures lie within the thermal limits of direct excitability of nerve fibres is a point which is apparrently yet to be determined.

Experimentation with the swallowing of hot and cold water (55°C and ice water) revealed again the ambiguity of the term heat. The observer was at first inclined to locate a heat sensation (two or three seconds after swallowing) at the lower end of the œsophagus. After further study and observation of external temperature sensations, however, he usually reported merely a glow of warmth, appearing late and due probably to diffusion to the skin, or a certain momentary, radiating sensation, internally referred (at about the level of the stomach), bright and hardly distinguishable in the two cases of warm and cold stimulus, sometimes described merely as 'contact', sometimes as similar to his cutaneous experience of 'heat' or weak heat-pain.[1] This sensation was usually absent on repetition and may of course have represented a peritoneal or diaphragm, rather than an alimentary mucosa response.

For the comparison of different kinds of pain—itch, smart, burn, ache, etc.—the application of a mustard plaster to a small area on the arm, a sharp rap on the palm of the hand with a ruler, and heat and cold stimulation were employed. Typical introspections for the first refer to itching as made up of intermittent pain sensations, fusing now into a sharp sting referred to a single point on the skin,[2] now spreading and irregular as if hundreds of little bubbles were breaking through to the surface; followed by a suffused warmth coming in waves along with pain of increasing intensity until a fairly unanalyzable mass of heat and pain results. For the second: "Smart made up of prickly points; gets more steady, a general ache or throb; ache is below surface, large, of one piece, rounder edges than smart." Similar observations on heat and cold pain, and the use of ginger, pepper, vinegar, lemon, etc., on the tongue, bring out the fact that the observers feel no need of different

[1] It seems to us highly probable that in the infinitely more painstaking experiments of Becher (*loc. cit.*), the 'temperature' sensations ascribed to the œsophagus were really of the nature of the sensations described above, due to the direct effect of comparatively high and low temperatures respectively upon internal fibres.

[2] This punctiform reference was not tested, but judging from other observations probably signifies only that pain was steady and sharp, not intermittent or dull.

qualitative terms in distinguishing any of these sensation experiences, even when for the sake of more exact comparison they followed closely in succession. Temporal qualifications, such as are implied in the terms intermittent, pricking, tingling, or intensive and spatial differences, seemed still to afford sufficient distinction. It is, however, to be noted that in connection with stimuli affecting either the mucosa, epidermis, or salivary glands the isolated sensation described as 'prick' or 'sharp' caused the observer some hesitation. While it is in itself neither painful, nor yet apparently possessing anything in common with the dull, massive quality usually identified with pressure, it stands in a sense between pain and pressure, since when intensified it passes gradually and insensibly into pain, and since also when an area of pricking sensations is allowed to die gradually away it merges into a massive dull sensation not dissimilar to pressure.

To sum up, we were left in considerable doubt as to the respective limits of the terms pain and pressure, the proper ranking of the pricking sensations, and their probable origin, whether in the skin or subcutaneous strata (in specific circulatory endings), in contractile or other changes in the walls of the smaller blood vessels. Further, we had so far failed to decipher any fixed qualitative variety among the pains of external origin which might serve as a guide or a clue in the analysis of internal pain. Lastly, a parallel series of casual introspections on visceral complexes indicated strongly that a thorough study of the external qualities and their different guises under the influence of intensive changes and reduplication, especially some investigation of the qualitative validity of the terms 'sharp' and 'dull' (which so far seemed to cover the most salient and distinguishing features of internally referred sensation), was a necessary preliminary to the successful analysis and description of the latter.

Significance of the terms 'sharp' and 'dull'; qualitative distinctions among cutaneous, subcutaneous and kinaesthetic sensations. A thorough study of certain variations of cutaneous qualities was accordingly undertaken, the results of which have already appeared in these pages, and may be summarized as follows. The typical simple cutaneous pressure (better *contact*) sensation, obtainable with moderate isolated stimulation of either pressure bulbs or 'pain' spots, is sharp or 'bright' in quality, and closely akin to the 'prick' which we had heretofore associated with stimulation by electric current and various irritants.[1] The granular pressure of Goldscheider is a

[1] XIX, 1908, 289 ff. A Qualitative Analysis of Tickling: Its Relation to Cutaneous and Organic Sensation.

complex, not a simple sensation, though qualitatively homogeneous. The ordinary dull pressure referred usually to lower strata, subcutaneous or muscular, appears under certain conditions of purely superficial stimulation of moderate intensity, as in the dying away of tickle. Tickle itself is not a peculiar organic quality, but represents a semi-analyzable pressure or contact complex and may be obtained directly from the isolated stimulation of a pressure bulb. The general bodily reaction conditioned by its irregular, flickering character, and strong dynamogenic power as an incompletely analyzed semi-cohesion of sensation probably accounts for the tendency to relate it to the organic or internal. Lastly, the dull pressure and the sharp prick above noted seemed with increase of intensity to pass into pain—dull ache in the former case, sting or sharp pain in the latter; the difference being probably a difference in massing and intensity merely. In a word, the results pointed to the possibility of reducing all the apparent multiplicity of sensation above cited, sharp and dull, to an astonishing qualitative simplicity.

Certain supplementary introspections combined with these experimental results, led to the formulation (in the above mentioned article) of the hypothesis that the whole muscle-skin continuum (barring pain, for the present) is qualitatively simple, and that its great apparent diversity is entirely explicable as a matter of 'form-color', or the peculiar temporal, spatial and intensive massing of similar sensations and the various resulting degrees of fusion, blurring, or flickering of the component parts.

In these introspections, of which unfortunately no considerable number was obtainable except from the writer, description was based as far as possible upon comparison with other more familiar or more easily designated sensations. The following descriptive summaries represent each from ten to twenty introspective studies made at odd moments for a period of about two years.

Gooseflesh. Arising from cutaneous stimulation—cold, the friction of clothing, nausea, rasping noises, and certain musical clangs. First, an apparently wave-like migratory sweep of sensation (after many repetitions analyzable into a multitude of vibratory or intermittent points), described at different times as pricking, seething, 'corkscrewy', 'whirring', bright, glowing, shadowy, 'shivery', or flickering, localized just under the skin, likened to a feeble faradization of the skin, to pins and needles, or to a light breeze over the hairs; later losing their sharpness and merging often into a drawn, tense feeling. Temperature sensations rare or feeble. After repeated analysis it seemed probable that the 'migratory' character at first noted was merely a matter of intermittence; careful introspection revealed no onward sweep of the sensation, though successive patches might appear. The curious, uncanny feeling-tone usually present seems to arise largely from the somewhat ghostly suggestion of movement in the absence of any visualizable stimulus. A similar complex of sensations, but much fainter, hardly sharp at all, fine, shadowy, arises

when a current of steam or compressed air is allowed to strike the face: presumably from the same cause, the movement of the fine hairs.

Pins and needles; numbness; circulatory sensations. The first (from falling asleep of the arm) consists, as in the case of faradization of the skin, of tingling or pricking sensations, sometimes so rapid as to merge into a mere whir or even a dull, pressure-like sensation, usually described as 'numb'; when sharper, frequently shading into a muscle ache, or a feeling of iron tension, which seems to split up again into sharp, needle-like sensations, if the limb is moved or the muscles contracted. The application of turpentine to the skin gives rise at first to something very like a low-toned ache, but giving away rapidly to boiling, seething sensations which seem to get faster and sharper, but never painful so long as attended to; or to a decided pulling sensation (or tautness) as in gooseflesh, which, however, always vanishes with analytic attention, hence apparently represents an interpretation of a certain degree of fusion and not a peculiar quality to be ascribed to the plain muscle fibres of the skin, as some have supposed. The sensations attending excitation of the salivary glands through certain odors or tastes show the same peculiarities,—lively, vibratory sensations grading into 'strain'.

Muscle, strain and joint sensations. The former we found very difficult to identify under experimental conditions (anæsthetization of skin and galvanization of muscles). What we obtained was either a continuous dull ache, outlasting stimulation (very possibly related to the use of the ether on the skin), or a shooting, jerking, quivering line of sharp sensation associated apparently with the contraction of a smaller muscle. We, therefore, began to look among casual instances of sensations referred (rightly or wrongly) to muscle regions—to fatigue, drowsiness, relaxation, numbness, the shock of fright, of sudden waking, etc.,—for a clue to the peculiar quality usually ascribed to muscle sensations by experimentalists.

It was first necessary to throw out the strain sensations, which we found readily identifiable, easily excited by voluntary muscle contraction, and accurately localizable, either directly or by pinching. They grade imperceptibly into pain, and are easily matched by pressing firmly over any bone or muscle. The remainder of the sensations referred to the deeper strata (apart from the pricking sensations already ascribed to pilomotor or circulatory changes) comprise a certain dull, massive, ill-defined 'tridimensional' feeling indistinguishable from pressure and present sometimes as a sense of the hugeness of the limbs in fatigue, in drowsiness or after a heavy sleep (when the peripheral capillaries are dilated). This sensation, the voluminousness of which is perhaps a suggestion from its mere aggregate character as representative of the stimulation of innumerable endings, may be in part cutaneous, but is present characteristically with prolonged massage, long after the superficial contact sensations have adapted out. It seems sometimes to be peculiarly unsteady (though this may be merely the effect of a shifting attention), and grades imperceptibly into a dull ache.

Certain other sensations are referred to the muscles, though possibly arising from circulatory changes, contraction of sweat duct fibres, etc. These are bright rather than dull, and arise *en masse* when we receive a sudden physical or mental shock—are angry, hurt, moved to pity, suddenly awakened from a nightmare, and the like. They are frequently sharp, 'corkscrewy', and fade quickly; the feeling as if 'boiled to shreds' described by James is apparently identical.

While it is, of course, probable that many of the sensations above described may originate in peculiar nerve-endings—muscle, tendon,

plain muscle, circulatory and the like—as it is certain that many of them originate in changes in the above-mentioned tissues, there is little warrant for assigning peculiar, individual qualities to such endings, since most of the characteristics of muscle, joint, skin, or tendon sensations may, under appropriate conditions, appear in the other tissues.

Varieties of Pain: dumpf and stechend, stabbing and cutting. With regard to this two-fold division, which is apparently accepted as ultimate and referred to different sensory systems (deep and superficial) by Nagel, Thunberg, Ebbinghaus, and probably Head, one or two points may be added. Pricking pain sensations may die away into a diffuse ache. A characteristic ache (deep, massive, and more or less dull) may, through changes in intensity or attention, swell out into a 'bright', almost sharp pain and die away again without otherwise altering in character, as embers may glow and dull again. It seems reasonable, therefore, to assume that here also the basis of division is merely a matter of spatial and intensive massing. Superficial nerve endings are, of course, more exposed to isolated and violent stimulation; deeper lying ones to massive and distributed attack.

Pain: The Concept of Indifferent Pain

Experimental. The immediate purpose here was to examine the nature of liminal and full-fledged pain sensations, their relation to the pressure (or contact) sense and its endings, and to determine whether this relation rises gradually along a graded series of stimulus intensities or springs suddenly out from painless sensation, as many assert. The results of these experiments, which were comparatively few and limited to the testing of pain spots with mechanical thermal and electric stimuli, have been already outlined in the article quoted. Two important facts disclosed were the existence of a graduated series of sensations ranging from vague indifferent punctiform pricks to pain (along a scale of graded stimulus intensities), and the uncertainty or instability of the pain judgment. Whether the same would hold for all pain endings or all stimulus forms we can hardly conjecture. With a fine pointed stimulus (needle) and a light momentary touch the response of a pain spot is frequently a tiny, *itchy*, irregular, apparently irradiating and delayed sensation, as under similar conditions the characteristic response of a pressure organ is tickle. In general, secondary or delayed pain (which at first we thought might represent the effect of vaso-motor reflexes in deeper endings) was obtainable from any pain spot if lightly enough stimulated; these delayed sensations are, however, strictly speaking, not always painful. Lastly, with intenser stimulation pain was obtainable from any pressure bulb tested.

Introspections and conclusions. Observations on the stinging or prickling sensations felt in the skin with light twisting massage, in the nostrils just preceding a sneeze, in pins and needles, and many other cases already quoted have revealed

sensations closely akin to the sensory aspect of pain, and yet unpainful, indifferent or even pleasant. Hence our inclination, already admitted, to credit the pain sense proper with a stretch of indifferent sensations in the direction of its lower limen, to regard the 'pain' quality as not alien to the pressure-contact-muscle-strain continuum, but simply representative of its upper stretches. It will, of course, be objected that in ordinary pain (*e. g.*, a needle prick) there is something peculiarly penetrating, peculiarly explosive, vivid, or stinging. Careful introspection, however, seems to resolve these peculiarities into a matter of abruptness of outswelling, of intermittence and apparent irradiation (in the case of itch), of complexity (in the case of tearing, wrenching pain), or into a matter of attentive vividness and general bodily reaction.

In cases of pricking or piercing sensations which seem to lie upon the border line, the observer, when doubtful, customarily basis his judgment upon his rebelliousness against the sensation. If now the natural reflex revulsion can be inhibited, or the sensory core singled out and dispassionately regarded by the attention, it temporarily loses its painful character and becomes merely an exceedingly vivid, live, sharply defined sense perception. Further, many sensations which, if momentary only are hardly painful, become so if continued, even when the sensation itself, if carefully noted, is neither intensified (by summation) nor otherwise altered. Neither is this a matter of an added diffuse unpleasantness, but rather of a complex dynamogenic effect. When the assault of any pain sensation which, if come upon unawares, would automatically grapple the attention and excite the impulse of avoidance or escape, is, as it were, headed off, the reaction inhibited, or some other substituted (as is possible only momentarily or not at all if the sensation is of considerable violence), the unbearableness, and with it the painfulness is lost.

It therefore seems to us that the pain consciousness represents neither a simple feeling or sensation,[1] nor a peculiar sense quality plus a simple and invariable unpleasant affective element, nor yet merely a peculiar sensation plus sometimes indifference, sometimes unpleasantness, but rather a peculiar miscellany or complex, an assimilation or welding of the immediate sense content (brilliantly illumined by attention) with a great bodily and mental reaction, the shorthand expression for which is 'intolerability'.

In taking this position we have, as above indicated, no intention of reverting to the earlier and cruder interpretation which,

[1]The *Gemeingefühlsempfindung* of the physiologists of the early nineteenth century; the *Gefühlsempfindung* of Stumpf.

confusing painfulness and disagreeableness, identified the pain consciousness as a peculiar 'feeling' and lightly posited the transmutation into pain of any specific sense quality or any specific nerve excitation—visual, auditory, temperature, or tactual. A concrete, specifically localizable aspect of pain is, we maintain, always present, and is sensory in the ordinary significance of the word.[1] Whether or not it is in many or all cases mediated by a specific end station and spinal pathway (*via* collateral connections in the cord), we are inclined to regard it as qualitatively alike in all senses and closely related to the indifferent prick.

In support of this concept of indifferent 'pain', certain theoretical considerations may be added. First, the facts of hyperalgesia and summation are hardly explicable upon the assumption of a high peripheral limen. Further, the concept enables us to account for the various sensations received from the viscera and inner tissues (painful, indifferent and pleasant) without a needless or difficultly intelligible duplication of nerve apparatus.

Summary. The net results of the above experimentation, so far as it bears upon the problems of organic sensation, may be summarized as follows:

1. The differentiation of external and internal sensation is less obvious, and the conditions of external or internal reference more complex than we at the outset imagined. A large number of internally referred sensations quite probably arise from the excitation of cutaneous nerves.

2. The texture or massing of sensation is as important in creating apparent qualitative differences (sharp and dull) as is the original sensory element itself.

3. Internal sensations may differ from external texturally rather than qualitatively, just as tickle differs from pressure merely from being so put together that it constitutes a 'feeling' rather than a sensation compact, is affectively vivid while perceptually vague.

4. The possibility of an indifferent beginning of the pain continuum offers a valuable suggestion for the solution of some of the problems of internal sensation.

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[1]That is, we assume that intense visual or auditory stimuli can evoke the pain reaction only when attended by veritable 'pain' sensations, by whatever mechanism aroused.

NOTES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE

I. Some Statistics on Synæsthesia

Collected by K. B. Rose

Two hundred and fifty-four women students, mostly from the Junior and Senior college classes, were asked to report upon any associations they possessed between colors or forms and letters, numbers, days of the week, months, and so on. It was found that 23, or a little over 9%, had color associations. Of these, 6 showed the phenomenon in a very striking degree; 7 in a moderate degree, and 10 in a slight degree. The order of colors arranged according to the frequency with which they entered into associations of this sort was, beginning with the most frequent: brown, yellow, gray, red, blue, green, pink, white, orange, violet, lavender. The colors were associated oftenest with letters, next oftenest with names of persons; then came names of cities, and lastly musical tones. Of the letters associated with colors 40% were vowels: since the number of consonants in the alphabet is about four times the number of vowels, this means a decided preponderance of vowels in color associations. The letter *o* was most frequently found in such associations: *a* was a close second, then came *e*, while *i* and *u* stood together as the vowels least often occurring in association with colors.

The number of persons having form-associations was 32, about 12% of the number questioned. In 27 cases the year was associated with a form, and in 22 of these the figure was that of an ellipse or circle, an obvious suggestion from diagrams of the earth's orbit such as are often found in geographies. In 21 cases the numbers from 1 to 10 suggested a form; in 16 cases the days of the week had this sort of association, and in two instances centuries had a figure associated to them.

II. An Instance of the Effect of Verbal Suggestion on Tactual Space Perception

Reported by M. F. Washburn

The observer in the experiments to be described was a young woman student of psychology, a good visualizer, and, according to her own statement, decidedly suggestible. The experimenters were the writer and Dr. Elsie Murray; probably the fact that they both, as members of the instructing staff, possessed prestige in the observer's mind, added to the effectiveness of their suggestions.

In the first set of experiments, the method was as follows. Rubber-tipped compass points, separated by a distance of 15 mm., were set down on the volar side of the observer's wrist, parallel to the long axis, and, after an interval of two seconds, set down again in the same region, being shifted only enough to avoid fatiguing the skin.

This procedure was repeated for ten experiments. The observer had been told beforehand that the points in the second of the two impressions would be either further apart or nearer together than in the first, and was asked to make the judgment 'larger' or 'smaller' with regard to the second distance. As a matter of fact, the same separation of the points was used throughout. Ten experiments made after this plan were followed by ten in which the observer was told that the second distance would be either larger than smaller than or equal to the first; the actual distances were kept constant as before. In a third series of ten, the conditions being otherwise the same, the observer was told that some of the second impressions would be of one point only. Eight complete sets, of thirty experiments each, were made. It was found that the judgments were distributed in the following way.

Suggestion	Judgment 'larger'	J. 'smaller'	J. 'same'	J. 'one'
Larger or smaller	28	42	5	5
L., s., or same	25	24	20	11
L., s., same or one,	14	16	26	24

The effect of suggestion is very striking.

In a second part of the investigation, the same suggestions were given, but instead of keeping the compass points at a constant distance apart, one point only was used in all the experiments. Ten complete sets of thirty experiments each were made. The following results were obtained:

Suggestion	J. 'larger'	J. 'smaller'	J. 'same'	J. 'one'
L. or s.	38	61	0	1
L., s., or same	23	42	33	2
L., s., same or one	22	34	28	16

Here the suggestion was still more efficacious. It is remarkable that the judgment 'one point' should have been given less often throughout this part of the investigation, where two points were never used, than in the first part, where one point was never used. After-images very likely played a part in producing the illusion of duality, although the skin was lightly rubbed between experiments to get rid of them. Twice, while the series where the suggestion 'Larger, smaller, same, or one' was being given, the observer expressed wonder that so few one-point impressions were given, although she was actually, of course, being given nothing else.

Finally, a single set of thirty experiments, ten in each series, was made where the impressions were given in accordance with the suggestions. That is, in the first set the second impression was sometimes larger, sometimes smaller than the first; in the second set, the two distances were sometimes equal, and in the third set both impressions were sometimes of one point only. There were five correct judgments in the first ten, six in the second, and six in the third, showing, as far as so small a number of experiments could, that the separation of the points was somewhat below the limen.

So marked an example of the influence of suggestion in this field, an influence which was first demonstrated by Tawney, seemed worth reporting.

SOME OBSERVATIONS UPON PRACTICE AND FATIGUE AS THEY AFFECT THE RATE OF TAPPING[1]

By ALICE M. BATTY

These observations were taken to determine how different periods of rest might affect the rate of movement in successive trials at taping and what might be the rate of practice gain in a series of observations taken for a number of successive days. The observations were made upon three regular reagents and the students who came into the laboratory by chance and for the regular laboratory work in the practice course. A single trial at tapping lasted for five seconds and five periods of tapping separated by different periods of rest were made to follow one another. The rest periods were five, ten or twenty seconds. Five trials at any given rest period are called an observation and six observations, three with either hand at the three different rest periods, constitute a record. Usually four observations were taken at a sitting, two with either hand, and only one sitting a day was allowed. The hands were alternated in beginning the successive observations and the tapping was begun each day at a rest period different from that at which it was begun the day before. In this way the after effects of previous tapping were as nearly equalized as possible. Upon reagent B_1 thirteen records were taken, upon B_2 sixteen and upon P_h twelve. The students made only a single record. Of the students there were two classes. The first class took their observations in irregular order, some beginning with one rest period and some with another. About an equal number began with each rest period. In the second class all began wth the twenty-second rest period, then followed the ten and finally the five. The mental after-effects of tapping with five second rest periods are felt for some time—several hours perhaps. While after-effects from the other periods are not observable for more than a few moments introspectively, they may still exist and affect the subsequent rates of tapping. If, however, records are taken with only one rest period, the practice effects, which are much more easily demonstrable than the temporary after-effects just referred to, will affect any subsequent work with the tapping test for many weeks. Consequently the plan of equalizing these various results by alternating the different rest periods was chosen.

The tapping was done upon a lever attached to the escapement of a clock. A hand was rigged to one of the wheels and this passed over the face of a dial. The time was taken with a stop watch. In the following table are given as general results, the average number of taps for either hand in the five successive trials and for all three rest periods. In one or two cases a figure was copied incorrectly so that the record was thrown out. Three observations from the records of P_h and one from the record of B_1 are wanting.

[1] This work was done under the direction of Professor T. L. Bolton in the psychological laboratory at the University of Nebraska.

TABLE I.

Reagent B1.

	Right Hand					Left Hand				
	5 sec. rest. 12 records.									
Trials.	1	2	3	4	5	1	2	3	4	5
Av. Taps	36.5	36.08	37.08	36.08	36.16	34.33	36.75	35.0	34.25	34.25
	10 sec. rest. 13 records.									
Av. Taps	35.69	38.53	36.3	36.07	35.61	32.84	35.3	35.07	34.61	34.0
	20 sec. rest. 13 records.									
Av. Taps	36.61	36.61	36.38	35.84	36.61	35.15	38.38	35.0	35.35	35.76

Reagent B2.

	5 sec. rest. 13 records.									
Av. Taps	38.15	37.0	37.0	36.61	37.76	37.46	36.84	36.15	36.07	36.53
	10 sec. rest. 18 records.									
Av. Taps	37.83	38.5	39.0	38.5	38.72	37.94	39.16	37.94	38.61	38.11
	20 sec. rest. 16 records.									
Av. Taps	38.0	38.31	39.25	39.06	39.31	39.56	39.0	39.37	39.87	40.81

Reagent Ph.

	5 sec. rest. 12 records.									
Av. Taps	43.75	43.08	43.41	43.08	43.16	44.41	43.25	42.83	48.75	41.33
	10 sec. rest. 12 records.									
Av. Taps	46.00	44.16	44.25	44.41	44.00	46.91	44.50	44.41	43.33	44.00
	20 sec. rest. 9 records.									
Av. Taps	42.88	42.11	42.22	43.22	43.88	45.55	44.44	44.44	44.33	44.22

Class Records. First Division.

	5 sec. rest. 38 records.									
Av. Taps	39.10	39.07	38.28	37.75	37.78	35.10	34.71	34.71	35.25	33.92
	10 sec. rest. 24 records.									
Av. Taps	38.25	39.25	38.95	39.66	39.33	36.41	36.20	35.87	35.37	35.58
	20 sec. rest. 26 records.									
Av. Taps	39.26	39.61	38.61	38.30	38.03	35.76	36.53	36.42	36.15	35.84

Class Records. Second Division.

	5 sec. rest. 31 records.									
Av. Taps	39.61	40.54	39.32	38.64	37.29	37.22	36.54	35.58	36.74	36.00
	10 sec. rest. 32 records.									
Av. Taps	40.40	40.62	39.81	40.06	39.03	37.03	37.18	36.62	36.40	36.78
	20 sec. rest. 32 records.									
Av. Taps	38.58	38.78	38.40	40.06	39.71	36.22	37.03	35.15	36.39	36.65

From these figures the average gain or loss in the five successive trials for all rest periods and both hands was computed. The average gain or loss for the five successive trials is given in Table II following:

TABLE II.

Reagent B1.

	Right Hand.		Left Hand.	
	Gain	Loss	Gain	Loss
5 sec. rest	——	.175 taps	——	.145 taps
10 sec. rest	——	.123 "	.161 taps	——
20 sec. rest	——	.067 "	——	.084 taps

Reagent B2.

	R. H.		L. H.	
	Gain	Loss	Gain	Loss
5 sec. rest	——	.114 taps	——	.307 taps
10 sec. rest	.177 taps	——	——	.022 "
20 sec. rest	.377 "	——	.337 taps	——

Reagent Ph.

	R. H.		L. H.	
	Gain	Loss	Gain	Loss
5 sec. rest	——	.110 taps	——	.900 taps
10 sec. rest	——	.375 "	——	.700 "
20 sec. rest	.311 taps	——	——	.277 "

Class Record. First Division.

	R. H.		L. H.	
	Gain	Loss	Gain	Loss
5 sec. rest	——	.470 taps	——	.182 taps
10 sec. rest	.258 taps	——	——	.266 "
20 sec. rest	——	.376 taps	——	.231 "

Class Record. Second Division.

5 sec. rest	——	.687 taps	——	.035 taps
10 sec. rest	——	.331 "	——	.153 "
20 sec. rest	.334 taps	——	.120 taps	——

The figures in this table show that when a five second rest period is allowed between the successive trials at tapping there is an average loss for all reagents. When the ten second rest is allowed, there is generally a less average loss than with a five second rest or there is an average gain. Two cases of the latter are found, the left hand of B_1 and the right hand of B_2. With a twenty second rest period the average loss is generally less than with either the five or ten second rest period, or there is a decided average gain. In the possible ten cases three times a greater loss is made with the ten and twenty second rest periods than with the five, and in two cases the ten second rest period shows a gain where a small loss is shown in the twenty second rest period. The evidence, however, that the twenty second rest is the most favorable pause between five second periods of tapping is fairly conclusive.

If the algebraic sum of the losses and gains for the three reagents and the two classes of students for each rest period is taken, the following figures are obtained:

	R. H.	L. H.
5 sec. rest	1556 loss	1559 loss
10 sec. rest	604 loss	1302 loss
20 sec. rest	739 gain	135 loss

The right hands show for the ten second rest less loss than for the five second rest and for the twenty second rest a considerable gain. The left hands show a loss decreasing inversely with the length of the rest period. The left hand thus responds less clearly to the favorable condition of a longer pause than the right. This is about what we should expect, although when the total amounts of work done by either hand and for all three reagents were computed, it was found that they are all practically ambidextrous. B_1 and B_2 are perhaps slightly right-handed and Ph is with equally small amount left-handed. While then the two hands have done about equal amounts of work, they have arrived at this result by different roads. Being less accustomed to voluntary direction, the left hand would not profit so readily by favorable conditions.

Another method of treating these results is here offered which shows well what has actually been transpiring. If the average rates of tapping for the successive trials for the three rest periods and both hands are taken separately and arranged by the ordinal numbers in the order from the highest to the lowest and the ordinal numbers are then added up, an expression for the place where the slowest rate of tapping is found will be obtained. Let this illustrate the method which we propose here to follow: For the right hand of reagent B_1 at the five second period of rest, the highest rate of tapping was found in the third trial and the next highest in the first trial, the fifth trial was third, the second was fourth and the fourth trial the lowest or fifth. The five trials for each reagent and the two classes are then arranged in the same way for both hands and for each rest period and the sums of the five columns are then taken. The result is given in the following tabular statement:

TABLE III

Five Second Rest

	R. H.					L. H.				
B_1	3	1	5	2	4	2	3	1	4	5
B^2	1	5	2	3	4	1	2	5	3	4
Ph	1	3	5	2	4	4	1	2	3	5
Cl I	1	2	3	4	5	4	1	2	3	5
Cl II	2	1	3	4	5	1	4	2	5	3
Total	8	12	18	15	**22**	12	11	12	18	**23**

Ten Second Rest

Total	12	**18**	14	14	14	7	14	17	**20**	18

Twenty Second Rest

Total	**17**	15	16	14	13	12	**19**	16	14	13

We have omitted the individual records and given only the sums of the five columns under the ten and twenty second rest periods. For the five second period the highest figures are found under the fifth column which means that the lowest rate of tapping was found here in the fifth trial. For the ten second period the slowest rate of tapping was at the second trial with the right hand and at the fourth trial with the left hand. For the twenty second period the slowest rate was at the first trial with the right hand and at the second trial

with the left hand. In other words the slowest rate has moved from the fifth place with the five sec. rest to the first place with the twenty sec. rest. This process of shifting has been slower with the left hand than with the right hand; it has lagged behind two places for the left hand with the ten seconds rest and it has reached only the fourth place with the left hand with the twenty sec. rest. Two main influences that act together to determine the amount of work that can be done within a given time and under given circumstances are fatigue and practice. Other influences pointed out by Kraepelin are beginning incitement, concluding incitement (finishing spurt) and warming up. If Wells's usage of the term "warming up" as an increase in rate of work during a period of continuous work rather than an increase which takes place between successive periods of work[1] is to be accepted and adopted, then none of the last three mentioned influences need to be considered here. Only interserial rather than intraserial changes have been studied here. While this distinction is well taken in the main, a further word for its broader usage will be offered. With the five sec. rest there is a continued loss in amount of work in successive trials. With the twenty sec. rest for the right hand there is a continued gain and for the left hand the gain begins only after the second trial. The five second rest is clearly inadequate to dissipate the effects of the fatigue of the preceding trial. Fatigue here outweighs practice or other accelerating factors. Twenty seconds seem fully sufficient except after the first trial with the left hand. Here there seems to be another phenomenon. The reagents complained of losing interest during the twenty second rest; they spoke of finding it difficult to keep themselves 'keyed up' for that length of time. The term "warming up" might then be made to cover this phenomenon of being keyed up or of not losing interest or excitement. Fatigue would still cover interserial gain, but it does not seem quite applicable for the gain showing after such short rest periods. With the ten second rest there is first a loss in work power and then a gain. This process of getting keyed up, excited or "warmed up" here takes some time so that at first fatigue overbalances any gain through practice, but later when the excitement has arisen, the speed of work increases with successive trials. When the average trial gain is taken, both hands show a loss, the right showing less loss than the left. The results in this last table show that the left "caught its pace" later than the right which harmonizes with the greater loss for the left than for the right.

The following table contains the average daily gains for the sums of the five trials.

TABLE IV.

Reagent	Hand	5 sec.	10 sec.	20 sec.
B_1	R.	3.234	2.027	1.494
	L.	2.770	2.410	2.500
B_2	R.	1.247	.566	.836
	L.	3.129	1.983	2.011
Ph.	R.	4.120	4.270	2.800
	L.	1.741	.923	1.360
	Total	16.241	12.179	11.001

The result is that the average daily gain is inversely as the length of the rest. The practice gain seems to be great in proportion as the

[1] F. L. Wells: Normal Performance in Tapping Test, *Amer. Jour. Psych.*, Vol. XIX, p. 446.

work is fatiguing. In view of this fact it may be said that, if the series were to continue long enough, the five second rest might prove the most favorable to the work capacity of the reagent. In Table III we might expect that for all rest periods the highest numbers would shift to the first places as the highest number is now found in the first place for the twenty second rest with the right hands. It is evident that in instituting comparisons between different grades of intellect, between classes of persons doing different kinds of work and between the normal and abnormal subjects, account must be taken of the practice efficiency in which the various subjects may find themselves at the time of the tests. Kraeplin's suggestion that we must study in such comparisons the manner in which the rates of practice gain are affected is in point here. Further work must be done to show how the curve of practice behaves and to determine the permanence of any practice effects that may be made and the likelihood with which they will appear in short and long series. Results gained from applying tests to different classes of subjects whose differences are alleged only upon the basis of common observation must be weighed with extreme care and conclusions drawn only with deliberate precaution.

In order to determine in what part of the series the greatest gain was made the observations were divided in the middle and the average daily gains were computed for each division. In the following table are given the algebraic differences between the gains or losses for the first and second halves. Where the second half has shown an advantage over the first half the figures are marked 'g' and where the result has turned out in favor of the first half 'l' is placed after the figures.

Right hand

Reagent	5 sec.	10 sec.	20 sec.
B_1	.233 g	.178 g	.025 l
B_2	.866 g	.358 g	.043 l
Ph	.566 g	.617 g	1.200 g
	1.865 g	1.153 g	1.132 g

Left hand

	5 sec.	10 sec.	20 sec.
B_1	.300 g	.133 l	.250 g
B_2	.530 g	.293 l	.214 g
Ph	.400 g	.100 l	.025 g
	1.230 g	.526 l	.948 g

All these records, except those for the left hands with the ten sec. rest, show a decided advantage in favor of the second half. In carrying out a piece of work fatigue makes itself felt first and practice gain disappears in counteracting the effects of fatigue. Practice consists in part in overcoming fatigue. The greatest advantage of the second half over the first is shown for the five sec. rest where the greatest average daily gain was made. It might be pointed out further that reagent B_1 who showed least fatigue loss gains least during the second half and reagent Ph who was most affected by fatigue shows here a decided advantage as gaining most during the second half.

The general conviction that practice gain is in proportion to the amount of fatigue engendered by the work is supported by the results. One must work up to his maximum, where he suffers most fatigue, that he may profit most by his work in the way of practice gain. The first object of practicing an effort is to overcome the depressing and

inhibiting effects of fatigue and even throughout any series of efforts a certain amount of the practice effect is lost through habituation of the reagent to the newer conditions imposed by the gains already made by practice. After a certain number of trials, however, the reagent is affected less and less by the mental symptoms of the work and the practice shows itself by the increased amounts of work done within a given time interval.

In conclusion it may be said that with five sec. trials at tapping the rest periods, five, ten and twenty sec. are favorable to work in an order the reverse of that in which they stand here and that these rest periods are favorable to practice gain in the order here given. With these rates of daily gain it is not improbable in a long series that the shortest rest period might prove the most favorable. It is suggested that before this test can be used as a basis of comparison for different classes of subjects tests must be made upon more subjects with only one rest period at a time, that they be continued for a longer time, and that other periods of tapping, ten and twenty seconds, be tried with the same and longer periods of rest. The standardization of the various psychological tests is imperatively demanded before further work in using them is done.

PSYCHOLOGICAL LITERATURE

Report of the Royal Commission on the care and control of the Feeble Minded, Vols. I-VIII. Wyman & Sons, London, 1908.

The most important contribution to the literature of mental defectives that has appeared for many years is the Report of the Royal Commission in England. This Commission, consisting of eleven men and one woman, was appointed by King Edward in 1904, "to consider the existing methods of dealing with idiots and epileptics, and with imbecile, feeble-minded, or defective persons."

This Commission has had a remarkable opportunity to study the problem and apparently they have made good use of the four years that they have devoted to it. They had full power to summon witnesses, visit institutions, and carry on investigations. They have utilized all of these methods, and have presented their findings in eight large "blue books" of which the eighth alone contains 512 pages. Five of these volumes contain the evidence relating to conditions in England, Scotland, Wales, Ireland, and the United States; one contains the Reports of Medical Investigations, one Appendices, and the last or eighth volume contains the recommendations of the Commission.

One of the most interesting chapters is that which discusses the cause of mental defect. The Commission did not consider an exhaustive investigation into this subject within their province, but they took much testimony from which it appears that the great weight of authority in England is for heredity as against environment. We had thought this point beyond question. It is the only view in accordance with the prevailing biological views and, besides that, it is practically certain that from 60 to 80% of all defectives have defective ancestry. There are, of course, a few cases of defect due to accidental injury to the brain, but these are comparatively rare and do not affect the main question.

Professor T. Clifford Allbutt says "I regard feeble-mindedness (if not accidental) as always hereditary." Dr. Bevan Lewis says "I look upon feeble-mindedness as a germinal variation as all variations are." Dr. Tredgold states that over 80% of mental defectives are connected with neuropathic inheritance. He has never seen a normal child born of two feeble-minded persons. Twenty-five physicians among the most prominent in England, and all dealing with large numbers of mental defectives, express similar opinions.

It must then be stated as the accepted opinion among those of largest experience, that feeble-mindedness is in the first instance a spontaneous variation, like a sixth finger or any other "sport", and as such is likely to be transmitted. The Commission sum up their conclusions on this point as follows.

(1) "That both on the grounds of fact and of theory there is the highest degree of probability that feeble-mindedness is usually spontaneous in origin—that is, not due to influences acting on the parents —and tends strongly to be inherited."

(2) "That especially in view of the evidence concerning fertility the prevention of mentally defective persons from becoming parents would tend largely to diminish the number of such persons in the population."

(3) "That the evidence for these conclusions strongly supports measures, which on other grounds are of pressing importance, for placing mentally defective persons, men and women, who are living at large and uncontrolled, in institutions where they will be employed and detained, and in this and other ways, kept under effectual supervision so long as may be necessary."

The "evidence concerning fertility" referred to is to the effect that mental defectives are about twice as fertile as normal individuals. Sixteen feeble-minded women in one workhouse together produced one hundred and sixteen children! The Commission concludes that the people would rightly condemn surgical and other artificial interference to prevent hereditary transmission of mental defect. Of twenty-one witnesses who mentioned the subject only three expressed opinions in favor of the practicability of such a course. As to preventing the marriage of defectives, they are equally conservative, and think it would be unwise to modify the existing law.

As to the number of mental defectives, their returns are rather startling. It has been generally held that two to every thousand of the population was a conservative estimate. The Commission find in Scotland 2.6 per thousand, in England 4.6, and in Ireland 5.7. If these figures are reliable, they furnish much food for thought and suggest many things in sociology.

The practical side of the work of the Commission is embodied in ninety-six recommendations for England and Wales, forty-three for Scotland and seventy-four for Ireland. H. H. GODDARD.

Das Gedächtnis; die Ergebnisse der experimentellen Psychologie und ihre Anwendung in Unterricht und Erziehung, von DR. MAX OFFNER. Berlin, Reuther & Reichard, 1909. pp. X: 238.

The chapter of Memory is one of the oldest in Psychology and one of the most voluminous in its literature. Even the experimental literature which began only twenty-five years ago with the work of Ebbinghaus has reached serious proportions. A work like this of Offner's which gathers up and co-ordinates with judicial fairness the results obtained by the large body of different workers is, therefore, particularly welcome. Though the book is intended in the first instance for teachers and for those who are entering psychology through their interest in memory, and for this reason has been kept close to essentials and purged of unnecessary technicalities, it will be found not to come amiss to the working psychologist whose labors have kept him in other fields and who wishes to put himself rapidly in touch with recent work on memory. The material is systematically presented and made further accessible by an analytical table of contents and full indexes. A bibliography of 181 titles, though not all upon the experimental studies of memory, is also included. The work as a whole is well calculated to fulfil the author's hope that it may promote a "comprehension of the methods of the newer psychology and confidence in them." E. C. S.

Ce que l'Armée peut être pour la Nation. Par le Lieutenant adjoint d'État-Major A. FASTREZ. Paris, Misch et Thron, 1907. pp. xiii, 294.

This work was called forth by Mlle. Joteyko's book on *Entrainement et fatigue au point de vue militaire*, 1905, and seeks to refute, on behalf of the Belgian army, the conclusion arrived at by its author and accepted by Prof. Richet, that the limit of military training is satisfactorily attained at the end of six to twelve months. M. Fastrez believes, on the contrary, that it is unwise to reduce the present term

of twenty months' effective service; he argues that Mlle. Joteyko has unduly narrowed the concept of training, and has overlooked the economic position and importance of the army.

The book opens with a chapter upon physical training, and its distinction from psychological. M. Fastrez then proceeds to outline the course of physical (including professional) training imparted to recruits and to appraise its results. He passes to the question of the psychological training of the soldier, and makes out a strong case for his side, mainly by reference to the events of the Russo-Japanese war. The psychological training which the recruit receives during his twenty months with the colors must be supplemented by similar training in the family, in the schools, and from the social environment at large.

M. Fastrez is thus led to consider the economic rôle of the army in the modern state. He points out, with especial reference to the history of Rome, the part played in the economic development of the ancient world by the peculiar circumstances of the military class, and declares that there is a close agreement, to-day, between the qualities of the trained combatant and the qualities of the economic worker. He shows that the maintenance of an army, as organ of defence, is implied in the struggle that pervades all nature, and particularly in the economic war that has come with the lessening of world-distances and the spread of imperialism. He ends with a paragraph on the resulting danger to small nations, which cannot afford to neglect any means for obtaining full returns, social, economic and 'energetic,' from the armed group of their populations. An appendix criticises, unfavorably, the outcome of the experiment recently made in England by Colonel Pollock and his 'Spectator' company. M. W. WISEMAN.

The Methods and Scope of Genetics, by W. BATESON. University Press, Cambridge, 1908. 49 p.

This address is a simplified statement of the theory of heredity as it has evolved in recent years under the influence of Mendelism, which has shed new light on the general thesis that everything that lives is not one thing but two, double throughout in every part of its composition. It teaches that we are assemblages or medleys of our parental characteristics, and establishes a law as to the prepotence of each of the moieties of heredity that come to us from the two parents. "If both parent-gametes brought a certain quality in, then all the daughter-gametes have it; if neither brought it in, then none of the daughter-gametes have it; if it came from one side and not from the other, then on an average in half the resulting gametes it will be present, and from half it will be absent." This last phenomenon, which is called "segregation," constitutes the essence of Mendel's discovery. The rest of the lecture gives a rapid, popular sketch of many special studies upon plants, animals and men that illustrate these general laws, which for the first time have given to variation and reversion a concrete and palpable meaning. "The time for discussing evolution as a problem at large is closed. We face that problem now as one soluble by minute, critical analysis." Variation is a definite, physiological event, viz.: the addition or omission of one or more definite elements; and reversion is that particular addition or subtraction which brings the total of the elements back to something it had been before in the history of the race.

Heredity, Variation, and Evolution in Protozoa, II, by H. S. JENNINGS. Reprint from Proceedings of the American Philosophical Society, Vol. XLVII, No. 190, 1908, pp. 393-546.

The author, who has made valuable contributions to the psychology

of simple forms of life, here presents us with a very painstaking series of studies showing growth of paramecium through seven stages, the effects of environment, inheritance of size, results of selection within pure lines, etc. It was found that large and small specimens of a single pure line produce progeny of the same mean size. The causes and nature of the variations in sizes even in a pure line must be many. Environment, especially nutrition, is very significant. A given wild culture generally contains many different races which maintain their relative sizes throughout all sorts of conditions. After many experiments it was found that selection within a pure line was quite without effect. Large individuals of the line produce progeny of the same mean size, as do small individuals. The variations found in many different individuals of the same pure line are not inheritable. The fact covered by this last sentence seems to be the most important of the author's results. Some elements of the environment increase breadth and decrease length, but most such elements change the breadth most. Any agent that causes rapid multiplication decreases the correlation between length and breadth. In general it would seem that selection is not effective within pure lines. In a mixed population, selection operates upon the various different lines already existing. Selection often will not carry a character beyond a certain point, because the line that has this original character strongly marked has been isolated, and selection of the fluctuations has no effect within the pure line. As to regression, the product of extremes stands nearer the mean than did the parents, although they diverge in the same direction. In such cases we have the largest individuals of the largest line, and the smallest of the smallest line producing the mean of the lines. Thus the largest and the smallest approach the mean of the original collection as a whole. As to the main question, how the different pure lines arise. we need further study.

Heredity and Prenatal Culture, by NEWTON N. RIDDELL. Child of Light Publishing Co., Chicago, 1900. 351 p.

This indefatigable author here pursues with great enthusiasm his own independent studies of heredity. His chapters are entitled: Some Objections Considered, Psychology, Brain Building and Soul Growth, The Reproduction of Life, the Factors of Heredity, Parental Adaptation, Sex Potency, Dual Parentage, Atavism, Prenatal Influences, Physical Preparation, Mental Preparation, Initial Impressions, Maternal Impressions, Maternal Impressions (continued), Abnormal Impressions; Heredity, Insanity and Imbecility; Heredity, Homicide and Suicide; Heredity and Commercialism, Heredity and Intemperance, Heredity and the Double Standard.

Mann und Weib. Edited by R. KOSSMANN and JUL. WEISS. Union Deutsche Verlagsgesellschaft, Stuttgart, Berlin and Leipzig, n. d. 3 v.

These three stately volumes are by more than a score of different authors, and are copiously illustrated with 421 cuts in the text and 22 illustrated inserts. In the first part man is described—his form, as child, youth, husband, father, widower, bachelor, his sexual and intellectual life—with eleven chapters by as many authors. Then comes woman in fourteen chapters. The work contains very little that is new for the scholar. Its chief merit consists in its voluminous illustrations and in its moderate price, 36 Mks.

Anti-Pragmatisme, par ALBERT SCHINZ. Félix Alcan, Paris, 1909. 309 p. (Bibliothèque de Philosophie Contemporaine.)

Although Dr. Schinz is a professor of modern languages, he has

long been known as a man of thorough philosophical training, wide knowledge, and very active mind. Those who read English will deeply regret that the author saw fit to write his work in French. It is the most masterly analysis and criticism that modern pragmatism has yet had. We regret that we are unable to devote more space to it and to give a more adequate review of it here. In the first part, the author takes up pragmatism and its relations to intellectualism, discussing its principles in general. A special chapter is given to Dewey. In the second part, entitled "Pragmatism and Modernism", the author shows the social phenomena that explain the principles of such a philosophy, and then considers the pragmatism of the Middle Ages and modern scholasticism. Scholastic metaphysics was the pragmatism of the Middle Age, and pragmatism is modern scholasticism. Not only scholasticism, but the pragmatism of Kant, the author deems indefinitely superior to that of James and Schiller. In one chapter, Dr. Schinz discusses the question whether James is a pragmatist or not; and leaves the reader to infer that neither James nor he himself is able to determine. In appendices, the author discusses the common sensations and philosophy, and the relations between literature and the moral code.

What is Pragmatism? by JAMES BISSETT PRATT. The Macmillan Co., New York, 1909. 256 p.

We have here six lectures given last summer at the Glenmore Summer School before an audience, if we understand the writer, of hardly more than half a dozen people, and the rest of the world is here compensated for its loss by being given these lectures in due form. No two writers have the same conception of pragmatism, and this makes it an admirable topic for those in our day who have a strong scholastic bent. The author tells at the outset of a law professor who discussed the question, whether the individual really owned his land or whether the state, which could exercise its right of eminent domain or could condemn it, was the party in whom ownership really vested. Pragmatism would say this was no problem at all, because ownership consists in enumerating the things the so-called owner can do. Pragmatism asks about everything what it means for me, for a thing is what it does. Meaning is influencing practice. Truth is what is useful or works well. Idea is a synonym for a plan of action. This is what Schiller calls humanism. It is the pet child of epistemology and gives speculators of this ilk a new and fascinating ambiguity to charge up against truth. Just as no pragmatists agree, so no two critics of it agree, and it is rather curious to see two books that have simultaneously appeared both disputing its claims, viz., Pratt and Schinz, taking almost diametrically opposite views of it. For himself, the writer confesses, that after having read much and, alas! written several papers concerning pragmatism, he is obliged to confess that there is such an incommensurability between it and the writer's mind that neither finds anything in the other.

Psychotherapy, by HUGO MÜNSTERBERG. Moffat, Yard & Co., New York, 1909. 398 p.

This is second in the series of books the author is writing to discuss for the wider public the practical applications of modern psychology. He promises others on sex, social problems, commerce, industry, etc. He tells us he has chosen "the form of loose popular essays;" and yet in the next sentence tells us there is too much loose talk afloat about psychotherapy. We are told that he has a personal right to deal with these questions because he studied medicine and holds the degree of M. D., and also gave the first university course on hypnotism in

the world, and since then "has never ceased to work psychotherapeutically." He has helped "many hundreds" and "no one ever had to pay anything." At the outset, we are given the psychological basis of psychotherapy, including the aim of psychology, mind and brain psychology in medicine, suggestion and hypnotism, psychology of the subconscious. The author develops his philosophical notion of a radical difference between the causal series of events which doctors chiefly concern themselves with, and the purposive series. In part II, he discusses the practical work of psychotherapy: its fields, general and special methods, mental and bodily symptoms. In the third, the place of psychotherapy, viz.: its relations to the church, the physician and the community. Barring the commingling of philosophical and scientific psychology, so characteristic of Harvard, especially protesting against the ever intrusive distinctions between causal and purposive, and the reiterated foible of the author that there is no subconscious or no unconscious, the book is on the whole a broad-minded and very sensible popular presentation of the subject. We confess, however, to some disappointment, after the many allusions in the past to this author's work, that the clinical cases he publishes are not richer, more numerous, and that some of them are so very fragmentary. Despite its defects and perhaps especially its diffusion, which it is very tempting to enlarge upon, its merits far preponderate; and it is on the whole the best popular presentation the subject has ever had in English up to date.

The Emmanuel Movement in a New England Town, by LYMAN P. POWELL. G. P. Putnam's Sons, New York, 1909. 188 p.

This is the third in a trilogy of books by this author within two years, the first entitled, "Christian Science;" the second, "The Art of Natural Sleep." The present shows the possibilities of the Emmanuel Movement as applied to a wide range of nervous functional disorders in the town of Northampton, and indicates the wider range to which the movement is destined to re-energize the whole church. The chapters are: What the Emmanuel Movement is; the Clinic in a College Town; a Year's Results; the Treatment of the Nerves; the Queer One in the House; the Cure of the Alcoholic; the Miscellaneous Cases; the Movement and the Church.

Letters on Psychotherapeutics, by PROF. H. OPPENHEIM. Translated by Alexander Bruce. Otto Schulze & Co., Edinburgh, and G. E. Stechert, New York, 1907. 60 p.

Here we have a number of interesting letters to various patients, some of whom have been eminent. There is certainly great sagacity shown.

The *Proceedings of the Society for Psychical Research* for Feb., 1909, contain an article by Sage on the Alleged Miraculous Hailstones of Remiremont, and two on D. D. Home by Count Petrovo Solovovo and Miss Alice Johnson, the former arguing against his employment of suggestion to produce illusions and hallucinations in his sitters, and the latter for this hypothesis. Miss Johnson's thesis is, in brief, that Home put most of his sitters through an educative process by which he made them highly suggestible, and she cites various instances, showing especially how he gradually worked up to his famous levitation feat of floating out of one window and in at another, eight feet above the ground. Count Petrovo Solovovo cites cases in which he thinks the proof is plain that the phenomena were objective at least, though they might have been produced by trickery, and he argues that since there was no hallucination in these cases there could have

been none in others. But Miss Johnson believes that Home employed whatever means were best suited to the particular time and place—sometimes suggestion, sometimes other methods of fraud. Mrs. Sidgwick also reviews fully Morselli's new book on Eusapia Paladino and Count Solovovo reviews Podmore's recent book entitled the Naturaliaztion of the Supernatural.

Text-book of Experimental Psychology, by CHARLES S. MYERS. Edward Arnold, London, 1909. 432 p.

At last we have a text-book of experimental psychology written in England by an Englishman. In this country we have had plenty. But this work is written on different lines. It presupposes some acquaintance with the elements of general psychology, such as Stout's work; but is less advanced than that of Titchener. It assumes, too, some acquaintance with the general structure and functions of the nervous system. Although the author lays stress upon the physiological and physical conditions, his ultimate object is to describe the methods, principles and results of psychological experimental research. He has not included the topics of animal behavior, or of children and primitive races, nor subconscious, abnormal states. The book is based upon experience in teaching. It begins with sensations—cutaneous, visceral, auditory, labyrinthian, visual, gustatory, olfactory; and then considers the specific energy of sensations, statistical methods, reaction times, memory, muscular and mental work, psychophysic methods, weight, local signature, sensibility and sensory acuity, identity and difference, and binocular and binaural experience, the visual perception of size and direction, and time, rhythm, attention, and feelings. There is an appendix of nearly one hundred pages in finer type at the end on laboratory exercises. There are in all sixty-six cuts, and a judicious bibliography at the end of each chapter.

Modern Educators and their Ideals, by TADASU MISAWA. D. Appleton & Co., New York, 1909. 304 p.

No text-book exists in English or in any other language save German (in which thirty years ago Vögel printed a volume on the educational views of the great thinkers in the history of philosophy who had treated the subject) on the history of the philosophy of education. The student in this field has hitherto been entirely dependent upon special monographs or upon gleanings such as he could make from the history of philosophy. This book attempts to fill this gap from Bacon and Comenius down to William T. Harris and Stanley Hall, both inclusive. The author is a special student of philosophy and psychology and his training and interest in both seem to be equally good and his sympathies well balanced between theory and speculation on the one hand, and empiricism and experimentation on the other. It is singular that it should have been left to a native of Japan first to attempt this work in the English language, and yet more singular that he should have succeeded in giving us in such lucid and idiomatic English the often abstruse topics of which the book treats. It cannot fail, in the opinion of the present writer, to find a speedy entrance into normal schools and educational departments of college grade. The references, in which the author has had the expert help of Dr. Theodate Smith, are well chosen. In our opinion the chief criticism of the book would be that it is not a little more detailed in some chapters. The writers treated, in addition to the above names, are Locke, Rousseau, Basedow, Kant, Pestalozzi, Fichte, Froebel, Herbart, Spencer, and Hegel.

The Life of the Spirit, an Introduction to Philosophy, by RUDOLPH EUCKEN. Translated by F. L. Pogson. Williams & Norgate, London, 1909. 68 p.

This is rather a charming group of five lectures with a conclusion, on unity and multiplicity, change and persistence, time and eternity, the outer and the inner world, problems of truth and of happiness. It is an easy and soothing work. Much of it is taken up with comprehensive, backward glances over the history of philosophy, in order to give the writer momentum of mind enough to come down into close and hard contact with contemporary topics of interest. The lecturer's chief anxiety is lest in the multiplicity of interests and movements to-day, the human mind shall lose something of its integrity, and unity take its departure from the world, discountenanced by many strident specialties. The author gives pragmatism a rather shady characterization through a number of pages, but does not go into it in detail. For those who are not familiar with the author's voluminous philosophical writings in the past, this will serve as an admirable introduction to them.

Elements of Ethics, by NOAH K. DAVIS. Silver, Burdett & Co., New York, 1907. pp. 294.

This is designed to give a simple, direct and comprehensive theory of morals, and to be a handbook for those pursuing a liberal education. It is the ripened fruit of a long life of teaching in this field, and the author, with due modesty, makes this the contribution of his experience. It is certainly lucid and an admirable introduction to the subject. The author has done a great deal of conscientious thinking and reading, being stronger in his knowledge of French and Latin than of German writers, which is, after all, perhaps no very great loss. The prolegomena has two parts, psychological and philosophical. His ethics also has two: the first, obligation, treats rights, liberty, trespass, law, sanctions, right and wrong, justice, beauty and virtue, selfishness, service, charity, welfare, and deity, each under a number of sub-divisions. The second part, organization, treats of man, the family, community, state and church. The last book in such a field ought to be the best available for college work, and we are inclined to think this book has met that high requirement.

Travail et Folie; Influences Professionelles sur l'Etiologie Psychopathique, par les DRS. A. MARIE et R. MARTIAL. Paris, Bloud. 110 p.

After an historical criticism, with various statistics as to the liabilities of insanity in the different walks of life, the authors discuss the development of medicine toward sociology in general and toward mental medicine in particular. In a chapter on inherent difficulties they discuss especially labor and then other causes of mental maladies among laboring people and how they manifest themselves. They then pass to a discussion of the types of mental disease most common to laborers in general, with a few conclusions concerning the contributions which certain of the great classes of laborers make to the specific disorders, with a number of tables.

Mental Fatigue and Its Measurement by the Æsthesiometer, by A. R. ABELSON. Engelmann, Leipzig, 1908. 147 p.

This appears to be a careful experimental work, but it is very hard to get into. There is no index of either topics or chapters, no general summary of the work, and the sixth chapter, entitled "Résumé", and generally a conclusion, does not tell us definitely just what the author himself claims to have found out. He is sure, how-

ever, that the loss of sensibility runs parallel with the conditions of fatigue. He thinks its curve rises quickly at first, then more slowly, until it hardly increases at all. Those who are nervous and weak show a more rapid rise of the curve; those who are strong and healthy will keep up for a longer time. When fatigue supervenes upon a generally exhausted condition and resistance is less, its curve rises higher in an irregular way. Still, weak persons often possess great resisting power until there may be a breakdown without any warning.

Human Physiology, an Elementary Text-book of Anatomy, Physiology and Hygiene, by JOHN W. RITCHIE. World Book Co., Yonkers-on-Hudson, N. Y., 1909. 362 p.

This little text-book is written after years of experience in elementary and more advanced classes, with the chief object of making physiology tributary to health. The author has tried to balance his book between the mere teaching of facts and the rules of health. Physiology, he thinks, is the teacher's science and it should be brought into relation to nature study and agriculture and to preventive medicine, treatment of emergencies, etc., so that he gives special chapters to tuberculosis, preventing the spread of disease germs, accidents, etc.

Psycho-Biologie et Énergétique, par CHARLES HENRY. A. Hermann et Fils, Paris, 1909. 216 p.

This vigorous writer here brings together many of his papers, which have appeared elsewhere under various headings, and gives them a certain unity. In Part I, under theory, he discusses principles, representative elements, multiple and complex representation, specialization of higher kinds. In part II, which treats of the applications to certain problems, he gives particular sections to balistics, the complete formulæ of the simple pendulum, the problem of a jet of water sustaining a weight, the dynamics of electrons, the law of Dulong and Petit, curves of growth, chemistry of respiration, sensations and energy, ending with conceptions of energy as applied to sociological problems.

Un Problème de l'Évolution. La Théorie de la Récapitulation des Formes Ancestrales au Cours du Développement embryonnaire, par L. VIALLETON. Coulet et Fils, Montpellier, 1908. pp. 244.

This work attempts to characterize the theory of the recapitulation of ancestral forms, particularly in the course of embryonic development, and especially to test the fundamental law of biogenesis according to Haeckel. To our mind, the author takes too many chapters in recapitulating what is rather well known of the history of this doctrine, and is naturally most interesting when in the later chapters he comes to his own work.

L'Evolution Psychique de l'Enfant, par le DR. HENRI BOUQUET. Librarie Bloud et Cie, Paris, 1909. 100 p.

The writer is a psychologist and specialist in infant maladies and here tries to sum up the results of his own long experience concerning the development of human mentality in the early years, following the earliest sensorial activities, touch, taste, hearing, etc., the development of language, the least and then more complex psychic activities, habit, memory, affective life, fear and imagination, fetichism, etc.

Rassentheorie und Geschichtsforschung, von FRANZ SCHOLZ. G. Scriba, Metz, 1908. 24 p.

The scientific and social idea of race is of recent origin but is now

making very impressive headway among learned men. This perhaps would hardly be seen in comprehensive scientific works, and so the writer attempts to develop it in these few pages, on the old and new method of writing history, the idea of race, the pre-historic man, the nature of kinds or varieties, the views of Gobineau, Chamberlain, and Lienhardt. The writer is a disciple of Gobineau in holding that the only real cause of racial decline is the mixture of bloods with alien and less valuable stocks. Christianity is said not to have helped mankind from a stirpicultural point of view and so is "not a civilizing agent" in this sense. The idea of race is based on natural history. Lienhardt thinks that the characteristic marks of a race are found only in the spiritual domain, and that physical traits are inadequate.

Erinnerung, Aussage und Lüge in der ersten Kindheit, von CLARA und WILLIAM STERN. J. A. Barth, Leipzig, 1909. 160 p. (Monographien über die seelische Entwicklung des Kindes.)

This very interesting and condensed presentation of many studies, in the first part treats of individual development and power of reproduction, telling how recognition is the forestadium of memory, and describes the sources of false statements. The second part treats the comparative psychology of the expressions of early childhood. This is treated from the standpoint of general observation and also from controlled experiments with single and prolonged exposures. Apparent and real lies are treated in perhaps the most interesting section of all. The practical applications bear upon observation and the power of reproduction, honesty, and the testamentary ability of children.

The Bawenda of the Spelonken, by R. WESSMAN. Trans. from the German by Leo Weinthal. "The African World," London, 1908. 154 p.

This is a contribution toward the psychology and folk-lore of the African people and describes life in the chief's kraal, family life, arts and industries, the salute, social laws and life, justice, mothers-in-law and sons-in-law, beer, various superstitions, customs, hospitality, proverbs, religion, rights, demonology, medicine men, witchcraft, African philosophy, the African war, ethnology, etc. The work is illustrated by perhaps two dozen photographs. The type is clear, and the author, from his long experience as a missionary, writes in an entertaining and very instructive way.

L'Adolescence, par GABRIEL COMPAYRÉ. Félix Alcan, Paris, 1909. 195 p.

The author has here written a work of fifteen chapters, based essentially on the work of Stanley Hall, whose name appears on nearly every page. The chapters are as follows: the duration of adolescence, physical growth, development of the organs, mental evolution, adolescence in literature, genetic psychology, feelings in adolescence, friendship and love, juvenile pathology and criminology, pedagogy of adolescence, the psychology and pedagogy of girls, co-education, the education of the young girl, the education of the young man. Thus essentially it follows Hall's order of topics and chapters.

Die sexuelle Not, von DR. FRITZ WITTELS. C. W. Stern, Wien und Leipzig, 1909. 206 p.

This work, which is dedicated to the author's teacher, Freud, discusses the following topics: Prohibition of abortions, Lust, Family, the Child—its ego, its rawness of *Gemüth*, its problems and cares, its voyages of discovery, and its sexual illumination. A chapter is given to the masculine woman, with two subdivisions: one on female crimes

and the other on female artists; and the final chapter is on the child-woman or the precociously beautiful and developed girl. The author writes with the knowledge of a physician, but in rather a literary style, so that his book has a good deal of attractiveness from this point of view.

The Psychology of Thinking, by IRVING E. MILLER. The Macmillan Co., New York, 1909. 303 p.

The writer once taught mathematics in a New England academy. Here he was forced into close touch with the actual mental processes involved in thought. Thus his interest in its processes became clinical, and it was this that turned him to the study of psychology. Although his work is colored with this dominant idea, it is also largely biological. Man's control over nature depends upon the higher, psychic processes we call thought. The author discusses the biological point of view, the sensori-motor circuit, the significance, function, differentiation and organization of consciousness, organic unity of mental and motor life, typical modes of adjustment, the condition and function of thinking, unity and diversity in the process, training in thought and the use of subject matter, the activity of the imagination in thinking, the image as an element of technique, the concept in its various aspects, induction and deduction, judgment, reason.

Fifty Years of Darwinism. Modern Aspects of Evolution. Centennial Addresses in honor of Charles Darwin, before the American Association for the Advancement of Science, Baltimore, Jan. 1, 1909. Henry Holt & Co., New York, 1909. 267 p.

This work contains an introductory lecture by Professor Chamberlain, Fifty Years of Darwinism, by E. Poulton; Natural Selection and Botany, by J. M. Coulter; Isolation and Evolution, by D. S. Jordan; the Cell, E. B. Wilson; the Influence of the Environment, by E. C. MacDougall; the Behavior of Unity Characters in Heredity, by W. E. Castle; Mutation, C. B. Davenport; Adaptation, Carl H. Eigenmann; Darwin and Paleontology, H. F. Osborn; Evolution and Psychology, G. Stanley Hall.

Analyse der Phobie eines 5-jährigen Knaben, compiled by Sigmund Freud. Franz Deuticke, Leipzig and Wien, 1909. 109 p.

Acquaintances of Freud working under his direction have studied a remarkable case of sexual precocity of a boy who at the age of three began to be interested above all things else in the Wiwimacher, and was eagerly concerned as to whether chairs, animals, men and women, etc., possessed this part, and could not be withheld from incessant interest and conversation on the topic. The child was plainly hereditarily *belastet*, but was cured by hypnotic treatment modified in form to be applicable to children.

Die Psychologie des Verbrechers, Kriminalpsychologie, von PAUL POLLITZ. B. G. Teubner, Leipzig, 1909. 148 p. (Aus Natur und Geisteswelt, 248. Bändchen.)

This is the most condensed treatise on criminal psychology known to the reviewer. Nevertheless, it is extremely comprehensive, touching most of the very many problems in the field. First comes all that can be taught from statistics; and then, in the second part, a special criminal psychology, such as the meaning of alcohol, juvenile crime, crimes of sex, tattooing, industrial crime, beggary, etc.

Life's Day, by WILLIAM SEAMAN BAINBRIDGE. Frederick A. Stokes Co., New York, 1909. 308 p.

This is a volume of Chautauqua lectures upon heredity, physiologi-

cal and psychical, environment, education, infancy, nursing and modified milk, childhood, the irresponsible age, defectives, the waif, adolescence, mid-day with notes on psychotherapeutics, twilight or old age, and night or death. The work is, on the whole, a wholesome combination of good sentiment and practical applications of modern science to the care of childhood.

The Psychology of Dementia Præcox, by DR. C. C. JUNG. Translated by Drs. Peterson and Brill. The Journal of Nervous and Mental Disease Publishing Co., New York, 1909. 153 p. (Nervous and Mental Disease Monograph Series, No. 3.)

Doctors Peterson and Brill have done a good service in translating these five chapters, which are entitled: a Critical Presentation of Theoretic Views on the Psychology of Dementia Præcox, the Emotional Complex and its General Action on the Psyche, its Influence on Association, its relations to Hysteria, and an Analysis of a Case of Paranoia Dementia as a parody.

Studies in Paranoia, by N. GIERLICH and M. FRIEDMANN. Journal of Nervous and Mental Disease Publishing Co., New York, 1908. 78 p. (Nervous and Mental Disease Monograph Series, No. 2.)

To this work Dr. Gierlich contributes an essay on periodic paranoia and the origin of paranoiac delusions; and Friedmann makes a contribution to the treatment of paranoia.

The Peasantry of Palestine, by ELIHU GRANT. The Pilgrim Press, Boston, 1907. 255 p.

The author lived nearly three years in Ram Allah, a village ten miles north of Jerusalem. The aim of the author has been to write only of matters that came under his personal observation, and to give a systematic description of village peasants, and also to contribute to the subject of Palestinian research and prepare the way for further study in the folk-life of the country. The author's chief object was to know his neighbors thoroughly; and he paid attention to everything, because everything interested him. To turn his journal into a book was an after-thought, but in doing so he has added scriptural references.

Ein Beitrag zur grammatischen Entwicklung der Kindersprache, von I. A. GHEORGOV. Wilhelm Engelmann, Leipzig, 1908. 295 p. (Sammlung von Abhandlungen zur psychologischen Pädagogik, herausgegeben von E. Meumann, II. Band, 3 Heft.)

The author has here brought together his new and interesting but very special studies upon this subject, which it is impossible adequately to describe without great detail.

The Epileptic Voice Sign, by L. PIERCE CLARK and E. W. SCRIPTURE. William Wood & Co., New York, n. d. 10 p.

Wilhelm von Humboldts Sprachphilosophie, von MORITZ SCHEINERT. Wilhelm Engelmann, Leipzig, 1908. 55 p.

Grundriss der Psychologie für Padagogen, von O. LIPMANN. Leipzig, J. A. Barth, 1909. pp. vi, 100. Price Mk. 2.

This little book, by the co-editor of the *Zeitschrift für angewandte Psychologie*, is intended as a practical aid to teachers, who have not the time to apply themselves to the special text-books of psychology. It thus forms the counterpart ofthe same author's *Grundriss der Psychologie für Juristen*, which appeared in 1908. Certain theoretical discussions are, indeed, identical in the two works.

The Preface contains a brief selected bibliography of general psychology, child psychology, and experimental pedagogy. The Introduction (1-8) then outlines the relation of pedagogy to psychology and ethics, defines the subject-matter and the method of psychology (laying especial emphasis upon experiment), and indicates the place and character of child-psychology within psychology at large.

The following sections treat of the various departments of psychology, always in the light of educational application. Section I (9-17) deals with certain laws of sensation, contrast, habit, adaptation, color-blindness, etc.; and then passes to ideas, their vividness, and their differences from sensation. Section II (18-35) takes up the intellectual functions: memory-span, association of ideas, illusion and suggestion, imagination, *Aussage* and children's lies, thought and the formation of concepts. Section III (36-48) continues these topics, treating of types of ideation, the laws of memory and of economical learning, the methods of recitation (question, dictation, etc.). Section IV (49-65) brings us to the affective life: sense-feelings, the higher ethical, intellectual and esthetic feelings, the emotions and the passions. Section V (66-76) turns, naturally, to volition, which is regarded as being, like emotion, a complex of intellectual and affective elements. The author here discusses the reflex, impulsive action, voluntary action, choice, character, inhibition; and ends with an essay on the pedagogy of conduct. Section VI (77-92) treats of attention, and the laws of practice and fatigue, bodily and mental. Section VII (93-98) touches briefly on the child's development in intellect, attention and interest, and on differences of native ability, and applies what is known of these matters to the problems of school-organization and of coeducation.

It is astonishing that Dr. Lipmann has been able to cover so wide a range of subjects in so brief a space, and has found room to say the many sensible things he has, on the side of application. The only positive criticism that occurs to the reviewer is that the treatment of attention should have come earlier in the book. It may be added that the work is far too abstract in its psychology for use by the American teacher.

TH. WALTERS.

NOTES

Preliminary announcement has been made of the celebration to be held in the coming September (6th-11th) of the completion of the second decade of work at Clark University. The celebration takes the form of a series of lectures by distinguished men of science and a series of conferences on the pedagogy of science in various aspects. The lecturers announced in the Departments of Psychology and Pedagogy are as follows:

Professor Sigmund Freud, University of Vienna.
Professor L. William Stern, University of Breslau.
Dr. C. G. Jung, University of Zürich.
Dr. Leo Burgerstein, University of Vienna.
Professor Franz Boas, Columbia University.
Professor H. S. Jennings, Johns Hopkins University.
Professor Adolph Meyer, Johns Hopkins Medical School.
Professor E. B. Titchener, Cornell University.

Conferences will be held upon the teaching of psychology in normal schools, under the chairmanship of Professor Guy M. Whipple of Cornell University, and on the teaching of psychology in higher schools for the training of teachers (teachers' colleges), under the chairmanship of Professor Carl E. Seashore, of the University of Iowa. A more informal conference on Laboratory Equipment and Management with demonstration of apparatus from various sources will also be held. Conferences are planned by the Department of Pedagogy upon Education as a Collegiate Subject and on School Hygiene in various aspects. Detailed programmes will be issued during the summer.

THE SIXTH INTERNATIONAL CONGRESS OF PSYCHOLOGY, GENEVA, AUGUST 3-7, 1909

From the second circular of announcement we take the following items of interest.

The regular session of the Congress will begin, nearly a month earlier than usual, on Tuesday, Aug. 3d, and continue until Saturday, Aug. 7th, inclusive. On Monday evening, Aug. 2d, there will be an informal gathering of the members of the Congress already present in Geneva.

The modifications in the plan of work of the Congress outlined in the first circular (*vide* this *Journal, XIX*, 1908, p. 286) have met with general approval and will be put into effect as indicated in the general scheme below, the detailed programme following later.

I. Themes for Discussion

A certain number of widely interesting questions—several of outside suggestion — have been placed upon the programme for special discussion. The papers of those who have agreed to bring these questions before the Congress will be printed and sent to the members of the Congress beforehand in order that they may be read at leisure and that members may prepare for their discussion. They will at the time of the discussion be presented in abstract only. The questions and leaders are as follows:

A. General Questions.

1. The Feelings, presented by Prof. O. Külpe (Würzburg) and Dr. P. Sollier (Paris).

2. The Subconscious, presented by Profs. Dessoir (Berlin), P. Janet (Paris) and Morton Prince (Boston).

3. The Measurement of Attention, presented by Profs. Patrizi (Modena) and Ziehen (Berlin).

4. The Psychology of Religion, presented by Profs. Höffding (Copenhagen) and Leuba (Bryn Mawr).

B. Special Questions — to be discussed probably in sectional sittings.

Psycho-pedagogy

5. The Psycho-pedagogical Classification of Dullards, presented by Dr. Decroly (Brussels) and Profs. Ferrari (Imola-Bologna), Heller (Vienna) and Witmer (Philadelphia).

6. The Methodology of Pedagogical Psychology, presented by Mlle. la Dr. Ioteyko (Brussels).

Psycho-zoölogy

7. Tropisms, presented by Dr. Bohn (Paris) and Profs. F. Darwin (Cambridge), Jennings (Baltimore) and Loeb (Berkeley).

8. Orientation at a Distance, presented by Prof. Thauziès (Président de la Fédération des Sociétés Colombophiles de l'Ouest-Sud-Ouest (Périguex).

Psycho-physiology

9. Perception of the Position and Movements of the Body and of the Members, presented by Prof. Bourdon (Rennes).

II. Questions of Unification and Standardization

1. Terminology. By way of introduction to this subject and to elicit suggestions, the Committee will soon publish and distribute a preliminary list of terms proposed, covering those most frequently used in experimental psychology.

2. Standard colors. The Committee here invites the assistance of psychologists and others expert in the matters involved. It is hoped that Prof. Nagel (Rostock) may bring the question before the Congress.

3. *Method of Enumerating Errors in Experiments on Testimony.* This matter will be brought before the Congress by Dr. Lipmann (Berlin).

4. *Notation for the Age of Children.* The Committee recommends the adoption of the method used by Stern.

5. *Mathematical Treatment of the Numerical Results of Experiments.* If it is desired, an opportunity will be given for the presentation of papers on this subject; and other questions can be placed upon the programme by prompt communication with the Committee.

III. Exposition of Instruments and Similar Material

Those who propose to exhibit instruments, apparatus, books, pamphlets, collections or other material, and those who will require facilities for demonstrations should notify the Committee at as early a date as possible.

IV. Individual Communications

The purpose of the Committee has been to restrict these, *pro bono publico*, but they do not wish to exclude them absolutely. In response to demand already made, a section for Animal Psychology has been

organized and communications are expected from Prof. Yerkes (Cambridge, Mass.) on Scientific Methods in Animal Psychology with a demonstration of apparatus, and from M. Hachet-Souflet (Paris) on the Theory and Psychological Applications of Training.

Those desiring to be enrolled as members of the Congress are invited to register at once with the Treasurer. The registration fee is 20 fr. covering, besides the opportunities of the Congress, the subscription for the Proceedings and all printed matter given out. Members of the families of those enrolling may obtain the privileges of the Congress, except the Proceedings and printed matter, at half-rates.

Committee of Organization

Th. Flournoy, President. P. Ladame, Vice-President.
Ed. Claparède, General Secretary. L. Cellérier, Treasurer.
E. Yung.

All communications, except those with reference to membership, should be addressed to the General Secretary, No. 11, avenue de Champel, Geneva. Communications with reference to membership and fees should be addressed to the Treasurer, M. Lucien Cellérier, Montchoisy, Geneva.

THE SIXTH ANNUAL MEETING OF EXPERIMENTAL PSYCHOLOGISTS.

At the invitation of Professor Howard C. Warren, the sixth annual meeting of Experimental Psychologists took place this year in the psychological laboratory of Princeton University, on April 8th, 9th, and 10th. Twenty-three psychologists were in attendance. The opening session was devoted to reports of work in progress in the chief laboratories. Of a somewhat more formal character was the demonstration by Prof. Leuba of his apparatus for the study of judgments of the extent of arm movements (described in an earlier portion of this number of the *American Journal*), the presentation by Mr. Geissler, of Cornell, of the results of his studies on the degrees of clearness in attention, the report of Prof. Hayes, of Mt. Holyoke, upon certain cases of color blindness (including one monocular case), and the summaries of Dr. Ferree, of Bryn Mawr, of his studies on spatial vision in the region of the blind-spot and on after-images and contrast due to subliminal stimuli. Mr. Clark, of Cornell, Mr. Woodrow, of Columbia, and Dr. Goddard, of the New Jersey State School for the Feeble Minded, reported respectively on the effect of distraction of attention on the apparent intensity of tones, the effect of variations in intensity and duration in the sounds in auditory rhythms, and the correlation between physical and mental development. Dr. Goddard also offered to the experimentalists opportunity for investigation in the institution to which he is attached.

Drs. Dunlap and Vaughan secured from the psychologists present an expression of opinion with reference to the suggested limitation of the scope of the *Psychological Index*, the prevailing sentiment proving to be against any limitation. In compliance with the invitation of Prof. Watson it was voted to hold the seventh meeting (1910) in the laboratory of Johns Hopkins University.

An agreeable social feature of the meeting was the Smoker given by Professor Warren at the Nassau Club on the evening of the 8th, which was followed by an informal gathering at the same place on the next evening. The personal hospitality of Professor Warren and his colleagues was also extended to many of the visiting psychologists.

We regret to record the loss to psychological science of the personality and trained productive capacity of Professor Hermann Ebbinghaus, of the University of Halle, who died on the 26th of February of pneumonia.

Prof. Ebbinghaus was born in Barmen, Jan. 24, 1850, and studied in the gymnasium of his native town and later in the Universities of Bonn, Halle, and Berlin. As his studies progressed he turned by degrees from history and philology to philosophy, and chose for the topic of his doctor's dissertation that of "Hartman's Philosophy of the Unconscious." Still later he applied himself to natural science and mathematics. The years from 1880 to 1894 he spent at Berlin as *Privatdozent* and extraordinary professor; he was called to Breslau as ordinary professor in 1894 and to Halle in the same capacity in 1905. While at Berlin he opened a psychological laboratory for research and for practice courses, and it was also while there that his classical experimental work on Memory was published (*Das Gedächtnis*, 1885). This work was a monument of patient and successful experimentation, and it was besides a demonstration that experimental methods could be applied with success to more complex fields than those of sensations and responsive movements.

In 1890 Prof. Ebbinghaus founded with Prof. Arthur König the *Zeitschrift für Psychologie und Physiologie der Sinnesorgane*, now grown to publication in two series and flourishing in its 51st volume. In 1902 appeared the first volume of his *Grundzüge der Psychologie* (2nd ed. 1904 and a 3rd in prospect), and recently the first *Heft* of the second volume. The contents of this second volume is foreshadowed in his *Abriss der Psychologie*, a concise and less technical sketch of the whole field of psychology, proposed originally for Hinneberg's "*Kultur der Gegenwart*", but used there only in abbreviated form and later brought out complete as an independent work (1908: 2nd ed. 1909, English version from the abbreviated form by Max Meyer, 1908). Beside these major works, he had published a considerable, but not a large, number of briefer contributions of which the best known are, perhaps, his experimental study of brightness contrast, his theory of color vision and his new method of testing mental ability in school children and others.

His number of papers was not large because in all his work he added to the qualities of the skilful man of science much also of the artist's reverence for his work—a characteristic which Jaensch's sketch in the *Zeitschrift* (Bd. 51, Heft 1-2, from which the above historical items are taken) shows to have been fundamentally characteristic of the man, and which causes us to regret his loss the more.